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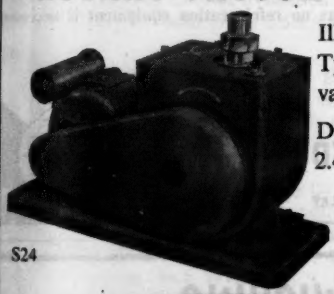

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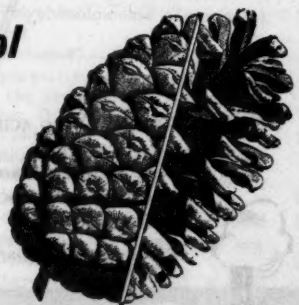

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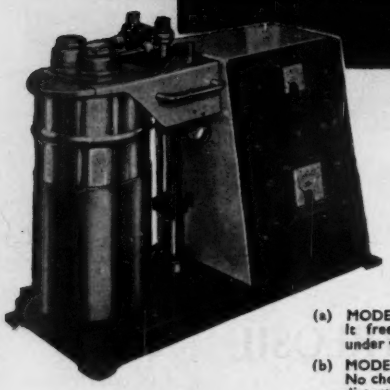

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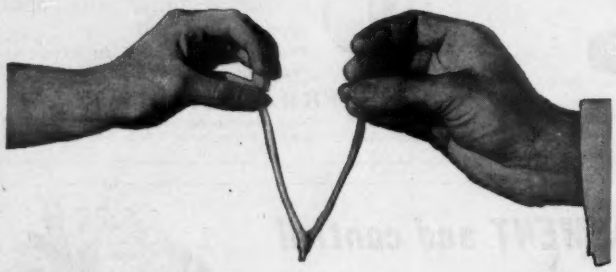

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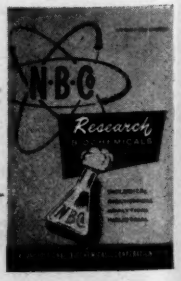

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PROGRESS IN UNDER-DEVELOPED COUNTRIES

THE committee appointed by the Council of the British Association to consider, under the chairmanship of Sir Ben Lockspeiser, the functions of the British Association in the world to-day, expressed the view that one of its purposes is to provide for non-specialists—whether they be scientists outside their own field or interested laymen—an annual stocktaking in intelligible language which will review the progress of science and help to make clear its significance and possible consequences. It is imperative, the committee held, that scientists and laymen should be seen to accept the need to anticipate and debate the social and political consequences of science. Moreover, such discussions should also serve to make it clear that modes of thought, other than the scientific one, must contribute to the solutions of problems created by scientific discovery.

Accepting this advice as wise, Prof. P. M. S. Blackett has not been deterred from following it in his presidential address by the inherent risk of becoming controversial (see p. 473 of this issue of *Nature*). That risk he regards as inevitable, because in science and technology lies the key to wealth, the creation and distribution of which are the warp and woof of the many-patterned texture of politics. Nor is this to be avoided by the rapid development of the social sciences for which Lord Adrian pleaded at Oxford in 1954. The facts discovered by social scientists, including economists, and the theories they elaborate, cannot by themselves, he points out, define the objectives of social policy. These goals must ultimately be determined by the judgment of value made by individuals. The social scientist may assist us to think rationally about alternative uses for our material wealth and may assist us to attain whatever end we choose. He cannot choose for us how much of the wealth resulting from technological advance should be devoted to the production of more material goods, and how much to more personal leisure and to gracious living; how much to providing greater consumption now, and how much should be set aside for the future; how much to greater opportunity for youth and how much to greater security or luxury for old age; or how and to what extent should different kinds of ability and enterprise be rewarded.

It is, of course, the different values attached to various objectives by particular groups or individuals that are responsible for most political controversy and tension to-day, both within a nation or on the international plane. The social scientist can sometimes, as Prof. Blackett observes, show us how best to attain our chosen end, and he may also help us to do so in such a way as to minimize friction and tension. It is no less true that the social scientist can warn us when the policy we seek to pursue in furtherance of our purpose is incompatible with either our resources or with the systematic observations which form the body of his science.

In this address, however, Prof. Blackett comments on the very uneven distribution of material wealth in the contemporary world and the attention that economists are now giving to the complex historical causes which lead a country to transform itself from a static pre-industrial State to a growing industrial one; and he goes on to discuss the question of how best the Western world can help to reduce a gap in wealth between the West and Asia which is steadily widening. The potentialities of nuclear power for the under-developed countries have not always been very realistically discussed. As Prof. Blackett remarks, most new scientific and technical discoveries or developments tend to widen the gap still more, just because the rich countries have the capital to make full use of them, whereas the poor countries have not. Under present conditions, the advent of nuclear power must tend to widen the absolute gap still further, because of the high capital cost of nuclear power stations, although it is of course in this context that the smaller nuclear power units of 30 MW. or less capacity may be important.

Be that as it may, Prof. Blackett is right to remark that scientific and technical invention is no fairy wand to wave over a poor country to transform it into a rich one. Apart altogether from the question of an adequate supply of technicians, technologists and able administrators, and of finding the necessary capital internally and externally, there are moral factors and judgments of value involved. Prof. Blackett points out that the saving and investment required for the initial industrial development in Britain were provided by the prosperous classes, who did not spend their surplus wealth on ostentatious living but invested it in productive industry. It might also be observed in this context of assisting the under-developed countries that the political conditions in Britain at the time encouraged such investment because of the confidence they inspired.

In glancing at the present position of the under-developed areas, and more particularly at what is being done in India to deal with the situation, Prof. Blackett notes that the limit of their advance is set mainly by the available capital for productive investment, and particularly of foreign exchange to buy production goods from other countries. Failing massive external aid, the advance is limited to what a democratic parliamentary government of a very poor country can raise internally by taxation and loans. Prof. Blackett admits the importance of the population problem and, though he notes that the governments of both India and China have already taken steps to reduce the increase in population, he recognizes also that success here can only be achieved

by education, which itself is dependent on increasing wealth and in turn on industrialization as part of a general social advance, with more efficient agriculture and thus a greater food supply.

Prof. Blackett's glance at the historic origin of the remarkable differences to-day between the economic and social conditions of the West and the East does not offer much guidance as to how to reduce the gap, though he notes the importance of the weakening of the opposition of organized religion to the free play of scientific experiment and speculation for the scientific revolution of the seventeenth and eighteenth centuries and the industrial revolution which followed in Britain. How important this factor can be even to-day is seen in the way in which political prejudice in Arabia impedes, and may even frustrate, international efforts on locust control. Possibly Prof. Blackett is inclined to under-rate the importance of this political climate in his confidence in the possibility of social advance in the ex-Colonial Asian countries and his reliance on massive financial help. He suggests that the need for technical assistance has been over-estimated, and that for simple financial help under-estimated. Whether, in fact, the expert aid by Western technical personnel needed by an under-developed country is more efficiently and cheaply provided by direct financial advance, leaving the country itself to engage the personnel it needs, depends a good deal on the vision and administrative character of the government of that country.

Prof. Blackett's lack of faith in technical assistance finds little support either in Dr. Gunnar Myrdal's study of the problems and prospects of an international economy published in 1956, or in the latest report of the Consultative Committee for the Colombo Plan (London : H.M.S.O., 1957), or of the Technical Assistance Board of the United Nations Economic and Social Council (United Nations, New York, 1957), although in general his argument as to the vital importance of massive assistance is fully substantiated. The Colombo Plan report stresses the need for flexibility in development and the continuing opportunities for adding to existing skills in these backward areas, for co-operation and further sources of external capital both from foreign private investment and public and private loans, while pointing, like Prof. Blackett, to the difficult decisions which will be required in assessing the extent to which the fruits of development can and should be devoted to consumption rather than investment. Specifically, it comments that the increasing effort in the field of technical assistance and the more varied ways in which this assistance is being given are removing a main impediment to economic development in the Colombo Plan area, and that technical assistance is more than a matter of helping to overcome a lack of technical personnel, and that its value cannot be assessed simply in statistical terms.

Equally significant is the comment in the report for 1956 of the Technical Assistance Board that the ability of a country to develop its resources and to take full advantage of technical assistance in any of its specialized fields is closely related to the stage of development of its basic institutional framework and of its administrative organization. Moreover, soundly based requests for technical assistance greatly exceed in volume the resources the Board at present has to meet them and even if, as is hoped, these resources increase, the Board contemplates some concentration in future development on the neediest countries and territories. Nor does the report give any impression that the available resources are not now wisely and efficiently administered. Much of the earlier criticism appears to have been heeded and overlapping eliminated so far as possible, although it would still appear possible for more to be done by the Board and by other agencies to establish an international service on which the under-developed countries could call for experts.

There are, of course, real difficulties in that direction ; but such a step would go far to meet the fundamental point which Dr. Myrdal makes, that assistance to under-developed countries, whether in the form of capital or of experts, should be given so far as possible through international agencies with responsibility for making policy. As to the value of technical assistance itself, Dr. Myrdal has not the least doubt. Even in the industrial field he considers it has a part to play and one which can only be effectively fulfilled on an international constitutional basis, as within the United Nations. In practice, it would be both psychologically and politically impossible to withdraw; and Dr. Myrdal agrees with Prof. Blackett that technical assistance is sometimes misunderstood—it is not, for example, a cheap way of assisting the under-developed countries—and points out that technical assistance is no substitute for credits and capital aid. In the main, the under-developed countries will have to pay for the industrial techniques and knowledge they require ; but technical assistance tends to mobilize the latent needs for capital, and the more it is made available and the more effectively it is applied, the larger, the better prepared and the more articulate will be the demands for capital.

Where Dr. Myrdal and Prof. Blackett agree even more emphatically is that an important contribution to narrowing the huge gap in material living standards between the industrial and pre-industrial countries outside the Soviet orbit could be made at relatively small cost to the rich industrial countries. Prof. Blackett puts the figure at an additional £1,000 million a year as a free gift or as long-term loans ; Britain's contribution of £150 million a year would only postpone by less than a year the expected rise of 50 per cent in British living standards over the next quarter of a century. Prof. Blackett is firmly of opinion that the West should make this great experiment of sacrificing some of its immediate prosperity to give massive aid to the under-developed countries ; and if the major nations of the West cannot agree quickly to provide the required £1,000 million, he believes that Britain should make her contribution of £150 million a year available to her

former Colonies in addition to what she is already doing.

Prof. Blackett sets this as a special challenge to scientists and technologists, since it is their genius and skill which alone can bring the material basis of happiness within the reach of all, though, as already suggested, he is inclined to pass over the more specific contribution which they could make in the field of technical assistance. There is much in his address to suggest that he is familiar with Dr. Myrdal's book, though he does not refer to the help which greater internationalism in university life and the gradual building up of a large number of research and training institutions in the under-developed regions could give—tasks in which the scientists and technologists can help as such, not simply as citizens. There is, moreover, a certain danger in the suggestion that Britain should act alone. Dr. Myrdal's argument for aid through international institutions rests in part on the desirability of a fairer distribution of the burden among the richer countries, and even if such unilateral aid did not encourage the political element and pressure politics, it could scarcely achieve the desired stability. Nevertheless, Prof. Blackett has challenged the British Association to some clear thinking and, however much his plan may require careful scrutiny and re-examination on points of detail, he will have earned the thanks both of the Association itself and of a much wider circle in the Commonwealth whose interests to-day the Association has re-affirmed its desire to serve, if action can be taken to prompt the imaginative Colonial experiments he suggests.

HARVEIUS REDIVIVUS

Movement of the Heart and Blood in Animals

By William Harvey. Translated from the original Latin by Prof. Kenneth J. Franklin. (English and Latin text.) Pp. xii+209. (Oxford: Blackwell Scientific Publications; Springfield, Ill.: Charles C. Thomas, 1957. Published for the Royal College of Physicians of London.) 17*s*. 6*d*.

THE plan of this work covers the following material: dedications to King Charles I and to other physicians; introduction; new English translations of the foregoing and of the seventeen chapters of the 1766 edition of "De Motu Cordis"; reprint of the Latin text of the same edition after revision of the punctuation and correction of typographical and editor's emendations.

In this English translation of Harvey's work on the circulation, Prof. K. J. Franklin had three predecessors, and his method was to make his own translation first, and then to compare it with those of his rivals. This is an admirable and courageous method, since it cannot be said that the result is only a modification or improvement on what has been done before. Robert Willis's well-known translation was published in 1847, and is indeed the "standard rendering to date", according to Franklin. Its literary style is grandiose and in places distinguished by a colourful imagination, but its scholarship is at times open to question and may even be difficult to follow. Prof. Franklin's work is therefore not only a new translation but is also undoubtedly free from the faults and shortcomings of previous authorities. In only a very few cases would one wish to modify his version. In the preface, when discussing the existence of pores in the ventricular septum, which would permit blood passing direct from the right ventricle into the left, Harvey says: "Sed me hercule porositates nullae sunt, neque demonstrari possunt", which is translated: "But, damme, there are no pores and it is not possible to demonstrate them". It may be doubted whether Harvey ever used this expletive, and the translation "by Hercules" given in contemporary Latin dictionaries is a more probable alternative and does in fact occur in Harvey's writings. Another passage, at the end of Chapter 16, is more important as giving the enemy occasion to blaspheme. Franklin describes certain veins as being 'distributed' to the stomach and intestine. The Latin verb used is not *distribuo* but *dissemino*, which is rare in classical Latin and should be translated not as 'distributed to' but as 'spread over' the gut. The rest of the paragraph makes it clear that the blood in these vessels is returning *to* the heart.

It is often stated that Harvey owes the discovery of the circulation to the work of his teacher Fabricius on the valves of the veins. This belief is not borne out by an examination of the plan of the 1628 volume. Fabricius himself did not discover the valves, which were known to Canano some sixty years before. Canano, however, did not publish what he had discovered or even write it up, but passed on his knowledge of the valves to Vesalius, who made some use of it in the second edition of the "Fabrica" published in 1555. In the first section of his own work, Harvey's concern was not with the valves of the veins but with the respiratory circle, where there are no valves. Having satisfied himself on the essential structure of this circle, he then continued his researches by an investigation of the systemic circle, and so completed his views on the circulation as a whole. In 1616, fourteen years after leaving Padua, he includes in the notes of his three Lumleian Lectures a passage of only eight lines, which, in spite of its brevity, is a clear statement of the circulation of the blood, to which he added the initials WH to indicate that the conclusion was his own. Another twelve years elapse before the idea is finally elaborated in the 1628 work. This prolonged period of gestation of twenty-six years gives but equivocal support to Boyle's description of an interview he had with Harvey in 1663. The great man then stated that it was the structure of the valves of the veins which first suggested "the circular motion of the blood". Harvey himself, however, when studying the history of his thought on the circulation, investigated the valves of the heart but ignored the valves of the veins.

Harvey's methods were simple, but, for his purpose, decisive. For magnification he used only a single hand lens, but this is not surprising since a complete compound microscope is not mentioned in the literature until 1621, and then its performance was less satisfactory than a single biconvex lens, as Leeuwenhoek showed later in the century. Harvey therefore had little knowledge of histology, and he followed Galen in his experimental work and Aristotle in his descriptive and natural history observations. His comparative studies cover an extraordinarily

wide field. In the three Lumleian Lectures he mentioned 128 types of animal life, forty-nine in his work on the circulation, and seventy-one in his volume on generation. He emphasizes emphatically that these comparative researches are of vital importance in solving questions presented by the higher animals. He practised vivisection on a large scale, and although he did not invent the ligation method of testing the course of the blood stream, he used it extensively in checking his own views on the circulation. He was always ready to exploit his beliefs by laboratory demonstrations to his friends, and to show them that Nature must be studied by way of experiment. He was therefore prepared to reject tradition when it failed to survive this ordeal. He claimed to be the only one to oppose tradition and to assert "that the blood travelled along a previously unrecognized circular pathway of its own". It must also not be forgotten that Harvey was the first to attack the problem of the circulation by quantitative methods, by which he calculated that the amount of blood discharged continuously from the heart in half an hour amounted to 500 ounces. This could not happen unless the stream moved in a circle. His figures have been condemned as excessive, but it is now known that in violent exercise the result is far greater. Blood capillaries were discovered by Malpighi four years after the death of Harvey, who, however, had already suggested that the finest arterial and venous twigs might be connected up by anastomoses. Harvey could have no knowledge of the oxygenation of the blood in the lungs, but understood that it could only discharge its functions as a fluid motile tissue.

Historians of medicine will be deeply grateful to Prof. Franklin for this learned and accurate translation of Harvey. He now joins the long and imposing list of eminent iatric scholars who have done so much to lighten the labours of their successors. They knew their Latin and were not unacquainted with Greek.

F. J. Cole

GALILEO ON THE PTOLEMAIC AND COPERNICAN SYSTEMS

Dialogue on the Great World Systems

By Galileo Galilei. (In the translation of T. Salusbury. Edited, corrected, annotated, and provided with a Historical Introduction by Prof. Giorgio de Santillana.) Pp. lviii+506. (Chicago, Ill.: University of Chicago Press; London: Cambridge University Press, 1957.) 94*s*. net.

THIS is the first modern English edition of the famous book that was to cause Galileo's conflict with the Inquisition. There exists an older translation by Thomas Salusbury, which appeared in 1661, but which soon became exceedingly rare. Hence the "Dialogue" has remained practically unknown to all English readers who were not able to read it in Italian. The Salusbury translation was very unreliable and obscure. This did not prevent the present editor, Prof. G. de Santillana, from using it as a basis for his own work. Rather than prepare an entirely new translation, he preferred to correct the existing one. It is not at all easy to understand what moved him to this decision. He tells us himself that Salusbury "every few pages . . . takes off on flights of inaccuracy that would make one doubt of his sanity"; he wonders "what the translator would have thought on rereading the nonsense he had written down". Nevertheless, "the Salusbury translation with all its faults, remains better (once corrected) than any modern one could be". This is a remarkable apodictical statement, which in my opinion is disproved at once by the excellent German translation made by E. Strauss in 1891.

However, we have to accept the translator's choice. The more so because the result of his recasting of a bad old translation turns out to be an excellent new one, in which the defects of the former have been put right: it is both correct and clear.

Of course, the text presents its own intrinsic difficulties. Seventeenth-century arguments make no easy reading for a modern scientist. These difficulties, however, are alleviated and to a great extent even removed by a large number of excellent explanatory notes, which are as many proofs of the editor's thorough knowledge of the history of science and of his admirable general erudition. He also provides a historical introduction, in which the deplorable tale of the vicissitudes the "Dialogue" caused its author is succinctly told. It is common knowledge that this subject has been dealt with in greater detail in the editor's recent book, "The Crime of Galileo" (Chicago, 1955). The value of the work is further enhanced by an interesting "Astronomical Note on the Two Systems", by W. D. Stahlman.

The only objection to be raised against this most valuable book is that it is somewhat deficient in references. To begin with, the reader is not told where he might consult the Italian text in a modern version (namely, in the seventh volume of the "Edizione Nazionale") let alone that a concordance with this edition should have been given. There is no list of other editions and translations. The title-page of the original edition is nowhere reproduced. The full title is only given in a footnote to the translator's (that is, Salusbury's) foreword, where it is rendered in an incorrect Salusbury translation. This leads to the regrettable consequence that the reader remains totally ignorant of the original wording of the title and therefore is not in a position to establish that throughout the whole book it is incorrectly rendered. It runs: *Dialogo di Galileo Galilei . . . dove ne i congressi di quattro giornate si discorre sopra i due Massimi Systemi del Mondo, Tolemaico e Copernicano. . . .* It should therefore be translated in an abbreviated form as *Dialogue of Galileo Galilei . . . on the Two Greatest Systems of the World, the Ptolemaic and the Copernican . . .* rather than *Dialogue on the Great World Systems.*

It must be regretted that no list of titles of the books quoted is given and that names of modern authors have been omitted from the index of names. I give one single example. On p. 62, n. 58, the name E. Strauss is given in parentheses. Who, the reader will ask, is E. Strauss and what has he to do with the "Dialogue"? The name is mentioned again on pp. 191, 240 and 284 (this, however, cannot be seen in an index). On the last page mentioned the irritating letters *op. cit.* are added, which only make sense if one can look up somewhere what the *opus* is and where it has been quoted. Finally, on p. 299, the name Strauss occurs again, and it is only here that the reader becomes aware that Strauss made a German translation of the "Dialogue". "Already quoted", the editor says. I must confess that I have not succeeded in finding the quotation; and the reader is still

ignorant of the title of the work and the year of publication. Yet the book in question is one of the most important items in the study of the subject. To the same category of bibliographical deficiencies belongs the bad custom often adopted by authors writing in English of quoting works written in foreign languages by an English translation of the title, in which the exact original wording often can scarcely be recognized. This is a deplorable usage. Titles of books are proper names and should be respected as such. They should always be given in the original form, to which an English translation may be added if necessary. Prof. de Santillana, however, quotes Galileo's work "Istoria e Dimostrationi intorno alle Macchie Solari . . ." under the laconic title "Solar Letters".

E. J. DIJKSTERHUIS

AIR AND WATER POLLUTION

Air and Water Pollution

The Position in Europe and in the United States. (Project Nos. 136 and 145.) Pp. 218. (Paris: European Productivity Agency of the Organization for European Economic Co-operation; London: H.M. Stationery Office, 1957.) 900 francs; 18*s*.; 3 dollars.

THIS document consists of the reports of two groups of experts selected from several European countries, including the United Kingdom, to visit the United States to study problems of air pollution (Mission 136) and water pollution (Mission 145). As a first step in 1953, Mission 145 visited industrial and other organizations in western European countries to obtain information on the problems of water pollution in those regions. The group included an expert from the United States, who then expressed the opinion that measures to mitigate water pollution in Europe were similar to those adopted in the United States, where there was little to be seen that had not already been studied by the Mission in the European countries visited. Considerable progress had recently been made, however, in dealing with air pollution in the United States and the developments there would be of interest to European countries, where much remained to be done. Air pollution then became the main study of Mission 136, which spent three months in the United States in 1954.

The first part, on water pollution, occupies about ninety pages and is divided into two main sections dealing with the legislative and with the technical and research aspects both in Europe and the United States. There are also references to international agreements between European countries and between the United States and Canada. On the technical side, there are descriptions of the methods adopted in dealing with polluting waste waters at municipal sewage treatment works and at the many kinds of industrial plants that were visited.

The second part, on air pollution, is similarly divided into two main sections, and there are subsections on the deleterious effects of air pollution on health, agriculture, and materials of construction. In one of four appendixes, there is a selected list of references to 187 publications on the subjects.

In referring on pp. 16 and 17 to experimental work on the Simon Carves–Central Electricity Authority process for removal of sulphur dioxide from chimney flue gases, the statement implies that all the experiments were undertaken at the Fulham power station. The early work was done at the power station, but the experiments with the pilot plants mentioned that led to the multi-stage process of scrubbing with a solution of ammonium salts were all carried out at the Fuel Research Station of the Department of Scientific and Industrial Research. This should have been clear from the original publications to which reference is made.

Another point is that in discussing the disposal of trade waste waters on p. 17, it is said that "where treatment is required to reduce oxygen demand, it is advisable that the effluent involved is passed to the nearest sewage treatment plant possibly during the night when the flow of sewage is at a minimum". In the experience of the reviewer, this is an exceptional method that would probably require a separate tank at the sewage works to receive the discharge during the night for mixing later with the sewage received during the day. The better procedure, and the one usually adopted, is to discharge the trade effluent to the sewer at a rate roughly proportionate to the sewage flow, that is, at a greater rate during the day than during the night so as to assist mixing with the sewage.

Apart from the points mentioned, the document in general is excellent and should be in the libraries of all concerned with problems of water pollution and air pollution.

A. PARKER

KINETIC THEORY AND GAS DYNAMICS

Molecular Flow of Gases

By G. N. Patterson. Pp. x+217. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1956.) 60*s*. net.

IN gas dynamics, four regimes may be distinguished. The first is that of isentropic flow, in which the gas is regarded as a frictionless and adiabatically compressible fluid. Next comes the imperfect fluid regime, in which viscosity and heat conduction are not negligible, and slip and temperature-drop at a wall may need to be taken into account. Then comes a transitional regime, in which the Knudsen number (ratio of free path to a length characteristic of the problem) becomes comparable with unity, as in strong shock-waves or low-density flow. Finally comes the regime of free-molecule flow, in which the Knudsen number is large, and collisions between molecules are unimportant. The first two of these can often be discussed adequately without reference to molecular structure; the others certainly cannot. In view of the increasing recognition of the importance of molecular structure in gas dynamics one is grateful to Prof. G. N. Patterson for a book discussing the effects of this structure in some detail.

The first and third chapters of the book, together with parts of the second and fourth chapters, are devoted to the (mathematical) kinetic theory of gases. Maxwell's velocity-distribution is derived for a simple gas, and Chapman's method is used for deriving expressions for the viscosity and thermal conductivity. Subsidiary topics treated are slip and temperature-drop at walls, bulk viscosity and relaxation effects. The remainder of Chapter 2 is devoted to isentropic motion, which is equated with motion in

which Maxwell's velocity-distribution function is valid. An explanation is given of the method of characteristics which is applied to one-dimensional expansion waves and steady two-dimensional flow around the outside of a bend. Chapter 4 discusses non-isentropic flows, particularly in a shock wave and in the boundary layer at a flat plate. The remaining chapter is concerned with momentum and heat transfer in free-molecule flow, and the effect of slip and temperature-discontinuity at a wall in ordinary boundary-layer problems. The Knudsen number is supposed either to be fairly large or fairly small. The transitional regime, in which this number is comparable with unity, is one in which the problems, though interesting, are mostly unsolved, and only one or two allusions are made to them. The treatment throughout the book is mainly mathematical, but there are numerous comparisons with experiment.

The book ranges over an enormous scale of subjects, and attempts to develop most of them from first principles. Inevitably this means that there are omissions and difficulties in the argument, which are sometimes serious when physical explanations are concerned. A reader without previous knowledge could not derive from this book alone a correct idea as to when flow is isentropic, and his ideas on the relation of relaxation processes to bulk viscosity could scarcely be clear. He would certainly wonder how to interpret Fig. 16 on p. 62, and would have great difficulty in ascertaining what problem is being considered in section 5.8. For such reasons as these I wish that the author had tried to cover less, and to spread himself more on the rest.

The book is undeniably stimulating, but to derive full benefit from it one needs to supplement it by further reading, for example, from the numerous papers to which references are given.

T. G. Cowling

VITAMIN A

Vitamin A

By Dr. Thomas Moore. Pp. xx+645. (Amsterdam: Elsevier Publishing Company; London: Cleaver-Hume Press, Ltd.; Princeton, N.J.: D. Van Nostrand Company, Inc., 1957.) 76*s*.

"IN 1915 McCollum and Davis took the initial step in the long journey towards the sub-division of the vitamins by postulating the existence of two factors, Fat-Soluble A and Water-Soluble B." Vitamin A was distinguished from vitamin D a little later, and thus it was found that cod liver oil which had been heated and aerated lost all its anti-xerophthalmic and growth-promoting power, but remained effective in curing rickets. Biological assays, mainly on rats, indicated a very wide distribution of vitamin A-active material in plant products and animal products such as butter, eggs, liver and especially fish-liver oils. An association between yellow colour and vitamin A activity was explored by Steenbock, von Euler and others, and it became clear that the plant pigment carotene was a potent source. Progress in another direction, however, had shown that the vitamin A-active fish-liver oils showed no parallelism between potency as measured by bio-assays and yellow colour. The vitamin A of liver oils was characterized by its ultra-violet absorption and by the blue colour test which it gave with antimony trichloride. In 1929 Dr. Moore himself carried out experiments which elegantly demonstrated that carotene is a precursor of vitamin A, in the sense that ingestion of this highly coloured fat-soluble hydrocarbon by animals deficient in vitamin A results in the appearance of colourless vitamin A in the liver.

Improved analytical methods, mainly spectrophotometric, led quickly to the discovery that some fish-liver oils were very rich sources of vitamin A, and also led to the recognition of a second active material designated vitamin A_2 and obtained most readily from the liver oils of certain freshwater fishes. Similarly carotene was sub-divided into α-, β- and γ-isomers. Organic chemists, led by Karrer, Heilbron and others, established the structures of the carotene isomers and of vitamin A_1 and that of vitamin A_2 followed after a considerable interval. It is possible to-day to purchase synthetic pure β-carotene and vitamin A at very reasonable prices. Behind this is remarkable technical achievement in which Isler played a big part.

The past thirty years have seen the fish-liver oil industry, once concerned only with cod, extended to halibut, shark and tunny and many other species. This expansion has served the general welfare by providing vitamin oils for human and animal prophylaxis. Synthetic vitamin A and carotene are added to margarine to the levels found in good butter. The part played by vitamins A and their aldehydes, retinene$_1$ and retinene$_2$, in the formation of visual pigments has become fairly clear but much less is known concerning the ysstemic mode of action of vitamin A.

The literature concerning this vitamin is now very large, but Dr. Moore's book is admirably selective. The chemistry of provitamins and vitamins A and various analytical problems are adequately described, and the author's experience and judgment are seen to great advantage in a very thorough discussion of the physiology and biochemistry of vitamin A and its provitamins. This section covers absorption, storage, mobilization and transfer from mother to offspring, as well as biological activity in relation to structure. Another section deals with the pathology of vitamin A deficiency or excess. This is followed by a long but very well-planned review of vitamin A in man. Chapters are devoted to special topics such as vitamin A in relation to sex, and vitamin A and the thyroid. The final chapter consists of a shrewd assessment of our present knowledge of vitamin A. This is followed by a number of useful appendixes.

This book must at once take its place as the standard work on a subject with many ramifications. The presentation is lucid, and the author does not lose sight of the fact that his readers will differ greatly in training and interests. Dr. Moore has tried earnestly to recognize 'the originator of every important discovery' in the field and in this task he has demonstrated fairness and sound scholarship. The book is illustrated with well-chosen photographs and diagrams, and each section is fully documented. Research workers interested in vitamin A from almost any angle will find in Dr. Moore a safe guide to the frontier of knowledge. He does not often venture far over the frontier, but the urge to do so can often be read between the lines of the restrained and modest narrative.

R. A. Morton

The Mechanism of Phase Transformation in Metals

A Symposium organized by the Institute of Metals and held at the Royal Institution, London, on 9 November 1955. (Institute of Metals Monograph and Report Series, No. 18.) Pp. iv+346+45 plates. (London: The Institute of Metals, 1956.) 50*s*.; 7.50 dollars.

DETAILS of the phase equilibria occurring in alloys represent a major foundation metal theory. The more difficult task is not so much to determine the actual phase conditions at equilibrium, but rather the mechanism of transformation from one set of conditions to another. In particular, it is the formation of various metastable products, *en route* to equilibrium or under conditions that do not allow the normal path to be followed, that provides such a diverse field of behaviour for investigation.

The present monograph is valuable because it gives in one volume a satisfactory summary of knowledge to date, which should prove useful not only to those with specific interest in the mechanism of phase transformations, but also for those who merely wish to keep up to date in a general way. The treatment has been classified broadly under two headings: first, those changes following a nucleation and growth sequence; and secondly, martensitic transformations involving, in contrast, a co-operative movement of the atoms. In each case a lengthy general review is followed by a number of shorter and more specific papers. Finally, there are more than sixty pages of general discussion. The matter is of high standard and forms a sound and stimulating account.

A. R. BAILEY

Records and Research in Engineering and Industrial Science

By Dr. J. Edwin Holmstrom. Third edition, rewritten and enlarged. Pp. xii+491. (London: Chapman and Hall, Ltd., 1956.) 60*s*. net.

UNCHANGED in title and in theme, the third edition of Dr. Holmstrom's book has been so substantially enlarged, re-arranged and re-written that it could reasonably be described as a new book. One chapter, dealing with the technician as a person, has been discarded, but neither in title nor in content are the nine that remain, apart possibly from the first, at all comparable with those in the second edition. The first chapter, in which Dr. Holmstrom discusses the nature and methods of technical science, illustrates one of the features in which the third edition constitutes a marked improvement on the earlier editions. It is characterized by a representative selection of references to outstanding post-war writings on this theme, and some thirty of the hundred odd additional pages of the new edition are occupied by an excellent bibliography. Dr. Holmstrom's reading is encyclopædic, but although he makes no claim to comprehensiveness, the basis of his selections either in time or in extent is not obvious and may be misleading to a reader. It is puzzling to find, for example, the "Dictionary of Applied Physics" and Kirk and Othman's "Encyclopedia of Chemical Technology" included, but not the "Dictionary of Applied Chemistry". Again, his references to organizations in the United States are strikingly incomplete in respect of publications of the past year or so, within which period he has given other references that might well have been too late to be included. There are also some surprising omissions in his account of organizations in other countries—for example, the National Foundation for Scientific Research, Brussels, and the Institute for Scientific Research in Central Africa. The book has been so greatly improved both bibliographically and in treatment of post-war developments that it would be a pity if readers were led to attribute to it a comprehensiveness to which the author himself makes no pretence.

R. BRIGHTMAN

Realities of Space Travel

Selected Papers of the British Interplanetary Society. Edited by L. J. Carter. Pp. 431+23 plates. (London: Putnam and Co., Ltd., 1957.) 35*s*. net.

AS part of its activities, the British Interplanetary Society publishes a journal containing articles relating to space-travel. A number of those which have appeared since 1948 have been collected in a single volume under the editorship of the secretary of the Society. The compilation forms an excellent introduction to various aspects of extra-terrestrial flight which should be understandable to anybody with some knowledge of elementary mathematics and physics.

Escape from the Earth's gravitational field, the dynamics of propulsion and certain aspects of orbit theory are fully treated. The problem of navigation is regrettably ignored.

Detailed designs of vehicles which might be used are rightly not presented. Attention is instead confined to basic features, such as rocket motors and wings for braking in descending through an atmosphere. The concentration on the question of what is possible in principle is to be commended. However, it might have been desirable to consider the extent of the practical engineering difficulties that would be encountered in the assembly of the satellite stations proposed by von Pirquet, in landing on and taking off from the Moon, or in one of the other major projects envisaged.

The survey is completed by interesting contributions on the following: instrumented satellites; meteor and cosmic ray hazards; physiological effects; air, water and food supplies for a voyage; conditions on neighbouring worlds.

There are numerous informative figures and tables and the general production is of a high standard.

D. R. BATES

Introduction to Structure in Organic Chemistry

By Prof. C. K. Ingold. Pp. vii+200. (London: G. Bell and Sons, Ltd., 1956.) 20*s*. net.

THE present volume consists of the first four chapters of Prof. Ingold's larger work, "Structure and Mechanism in Organic Chemistry" (1953). Dr. W. A. Waters, in a highly commendatory review of this earlier book (*Nature*, 173, 278; 1954), called it a "classic of scholarship". Chapter 1 deals with covalency, its physical interpretation and electronic character, and with the interpretation of stereochemical form; Chapter 2 with interactions between non-reacting molecules, and also with intramolecular interaction, its significance and its internal transmission; Chapter 3 with electric dipole moments, and with the physical properties of molecules; Chapter 4 with the theory of aromatic character, and with the diamagnetic and other physical properties of the aromatic nucleus. The book is therefore essentially an exposition of the general principles of molecular structure with all the illustrations taken from organic chemistry.

G. FOWLES

THE THREE TYPES OF EVOLUTIONARY PROCESS

By Dr. JULIAN HUXLEY, F.R.S.

THE three major types of process operating in evolution are those leading to divergence and variety, to adaptedness and biological improvement, and to stabilization and persistence of type. Darwin[1] before 1859 had realized the universality of the first two processes, and had noted some examples of the third, the importance and widespread occurrence of which was stressed by T. H. Huxley[2] in 1862. However, from then on to the end of the century most biologists were concerned with establishing the fact of evolution and the evolutionary interpretation of comparative anatomy, taxonomy and adaptation; while the period 1900–30 was chiefly devoted to establishing the mechanism of evolution–natural selection on the basis of particulate variation and inheritance. Only since then has attention been focused on evolutionary process.

The time has now come to define the three major types of evolutionary process more strictly. I accordingly propose the terms *cladogenesis*, *anagenesis*, and *stasigenesis* for the processes leading to divergence, improvement, and persistence respectively. *Cladogenesis* I have taken over directly from Rensch[3], to denote all splitting, from subspeciation through adaptive radiation to the divergence of phyla and kingdoms. Rensch[3] has used *anagenesis* to denote advance in general organization or perfection of some major function: I propose to generalize it to cover all types or degrees of biological improvement, from detailed adaptation to general organizational advance. The universality of persistence has not been properly recognized: I therefore propose the term *stasigenesis* to cover all processes leading to stabilization and persistence of types and of patterns of organization, from species up to phyla.

In passing, evolutionary biology is also handicapped by the absence of a suitable chronological terminology. We still specify evolutionary time in terms of millions or hundreds of millions of years. This is as inadequate as if we were forced to describe individual human age in terms of millions of seconds, or human history in terms of hundreds of millions of minutes. I therefore suggest the term *cron*, simplified from χρόνος, to denote 1 million years as the basic unit of evolutionary time, with derivatives such as *kilocron* for 10^9 years and *millicron* for a millennium.

The result of evolution has been to produce a number of delimitable taxa or recognizably distinct groups of organisms, each characterized by a particular pattern of organization, which can be further grouped in a hierarchical arrangement—species, genera, families, orders, classes, and phyla. When fossil evidence is available, taxa, notably those of larger extent, often show succession and replacement, a once abundant group becoming replaced in its successful evolutionary position by a later group, often one to which it has itself given rise. The replacement may either be total, eventually involving extinction as well as reduction, like that of the *Archæornithes* and the toothed birds by modern birds, or partial, involving only reduction, like that of Reptilia by Aves and Mammalia. The earlier group may show extreme reduction, only a few 'living fossils' surviving, as with *Dipnoi*, *Limulus*, *Nautilus* and the recently discovered archimollusc *Neopilina*[4].

Almost always the reduced group persists for a considerable time, and often indefinitely, after the emergence of the new and more successful group. Furthermore, even the latest-evolved and most successful (dominant) groups show stabilization and persistence through tens of crons within certain defined organizational limits; for example, Aves since the Miocene, and Formicoidia since at least the Oligocene.

Here we see stasigenesis at work. Evolutionary success appears to depend on the acquisition of an integrated plan of organization, which shows evolutionary homœostasis or stability in confining variation within the limits of its general pattern. Only in exceptional circumstances will the emergence of a new stabilized pattern be possible. This applies at all levels. Mayr[5] has shown that it applies at the species-level. In the 'explosive' period of a group's evolutionary radiation, many divergent subgroups are produced, only the better integrated adaptive types surviving[6]. At still higher levels, a successful group like the Reptilia produced a major radiation of types, but almost all remained within the reptilian pattern: only two lineages, those leading to Aves and Mammalia, achieved a new stabilized organizational pattern capable of showing further major radiation.

Intrinsic restrictions, depending on inherent properties, may also operate to set an upper limit to the range of evolutionary variation possible to a group: for example, size and speed in crustaceans, insects and terrestrial vertebrates, visual performance in vertebrates, arthropods, etc. Only when the unique

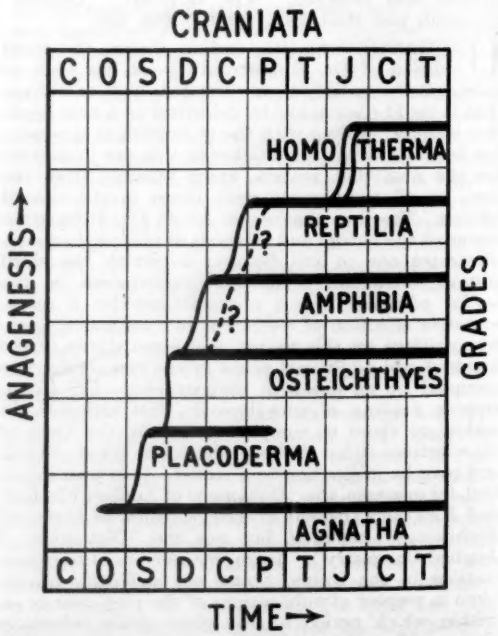

Fig. 1. Anagenesis and stasigenesis in vertebrates, leading to a succession of persistent grades. *T*, Tertiary era: other initials indicate geological periods from Cambrian to Cretaceous

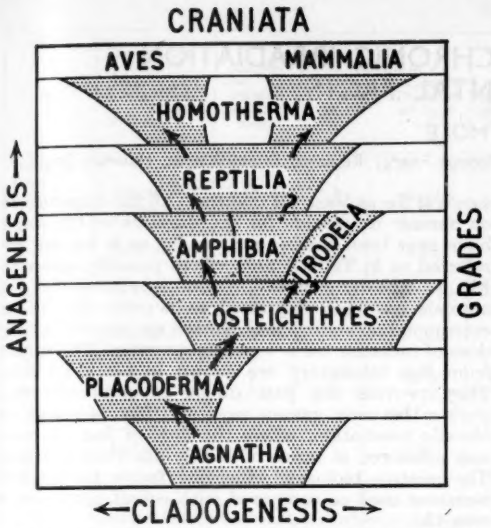

Fig. 2. Anagenesis and cladogenesis in vertebrates, with time eliminated. Some grades are also clades (monophyletic), others possibly or certainly diphyletic

emergence of man had made possible exosomatic evolution, and with it new materials and new types of organization, could these limits be transcended.

Anagenesis also operates at all levels to produce the improvement of evolutionary trends of every scale. As Darwin clearly stated[1], natural selection inevitably promotes biological improvement, but always "in relation to the conditions of life". Thus the description and analysis of anagenesis must always be relative to the scale and nature of the particular trend under consideration, whether detailed adaptation to a specific niche, specialization for a particular mode of life, advance in efficiency of a major function such as hearing or homœostatic regulation, advance in general organizational level, or progress in the sense of advance which does not stand in the way of further advance[7]. The reduction or loss of certain organs can properly be called anagenetic in respect of specialization for a particular mode of life: for example, the loss of digits in higher ungulates or the 'degeneration' or loss of organs in certain parasites.

What I have called 'advance' and 'progress' may be measured or at least estimated by increase in control over environment and in independence of environmental change[7]. We may require special technical terms for improvements of different scale and relevance, such as 'advance', 'progress', etc.; here I am only concerned to point out the need for a single term to cover all types of improvement.

Cladogenesis operates primarily by a combination of isolation and natural selection, with occasional contributions from drift and similar genetic phenomena. The increased variety which it generates is itself from one point of view an anagenetic improvement, since (as Darwin, also clearly pointed out) it permits a fuller exploitation of the environment.

All three processes co-operate in evolutionary phenomena of all scales from the species-level upwards. Cladogenesis results in the formation of delimitable monophyletic units, which may be called *clades*; and anagenesis results in trends of improvement. But cladogenesis always involves some degree of improvement (anagenesis): this is most obvious in adaptive radiations which produce many specialized lines, but occurs also in divergent species, which are always to some extent adaptively differentiated. Finally, stasigenesis results in the formation of delimitable and persistent anagenetic units, or *grades*, as is obvious when anagenetic improvement is plotted against time (Fig. 1). Most delimitable taxa will thus be simultaneously both clades and grades: on a two-way graph with time eliminated and anagenetic improvement plotted against cladogenetic divergence (Fig. 2), they will appear as *evolutionary areas*, and the direction of evolutionary trends will almost always be oblique, the resultant of cladogenetic and anagenetic processes in combination.

In point of fact, taxa are usually recognized as grades which have been observably delimited by the fact of their stasigenetic persistence. Save in the exceptional event of abundant fossil evidence, phylogeny is not observable and must be deduced, so that the phylogenetic relationships of taxa are much more speculative. Unfortunately, however, since "the avowed aim of a modern classification is to reflect phylogeny" in which "the categories are monophyletic"[8], the conscious or more often unconscious assumption behind most systems of classification is that named taxa are or should be monophyletic clades, even though in their work taxonomists often adopt more pragmatic methods[9]. In practice, however, a number of named taxa have turned out to be certainly or probably polyphyletic (for example, the 'subkingdom' Metazoa, the 'phylum' Bryozoa, the 'order' Teleostei[6], the 'family' Miacidae, the 'genus' Merychippus): they are grades, but not clades.

In general, the implications of the three co-operating processes of evolution have not been made explicit, and the fact that anagenesis and stasigenesis in combination produce delimitable gradal units has not been fully taken into account.

Taxonomy will have to envisage a two-way system of classification, which will do justice to the facts of biological improvement and persistence of types as well as to those of phylogenetic divergence[9]. Such a system must recognize that many delimitable taxa are simultaneously grades and clades, and that others are grades which may or may not also be single clades. This will presumably involve some new terminology, though not necessarily subject to the international rules of nomenclature. Some new purely gradal terms will be required: for example, Homotherma for Aves plus Mammalia, and Psychozoa for man. This last example is illuminating. Cladogenetically man constitutes only the single family Hominidae: but anagenetically he constitutes a grade equivalent in evolutionary importance to all other organisms taken together.

[1] Darwin, C., "Origin of Species", 5th ed. Ch. 4, pp. 96, 131, 143–5; ch. 10, p. 386.
[2] Huxley, T. H., "Geological Contemporaneity and Persistent Types of Life", in "Collected Essays", 8 (London, 1894).
[3] Rensch, B., "Neuere Probleme der Abstammungslehre", 2nd ed., pp. 103 and 302 (Stuttgart, 1954).
[4] Lemche, H., *Nature*, 179, 413 (1957).
[5] Mayr, E., "The Evolutionary Significance of the Taxonomic Categories", in Linnæus Symposium, Uppsala Univ. Årsskr. (in the press, 1957).
[6] Simpson, G. G., "The Meaning of Evolution" (New Haven, 1949); "The Major Features of Evolution" (New York, 1953).
[7] Huxley, J. S., "Evolution, the Modern Synthesis", ch. 10 (London, 1943).
[8] Mayr, E., Linsley, E. G., and Usinger, R. L., "Methods and Principles of Taxonomic Zoology" (New York, 1953).
[9] Huxley, J. S., "Evolutionary Processes and Taxonomy, with Special Reference to Grades", in Linnæus Symposium, Uppsala Univ. Årsskr. (in the press, 1957).

SHORTENING OF LIFE BY CHRONIC IRRADIATION: THE EXPERIMENTAL FACTS

By R. H. MOLE

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IT is probably true to say that more is known of the biological effects of radiation than of any other environmental hazard except bacteria. Certainly the chronic toxicity of no chemical substance has been investigated as thoroughly as the chronic toxicity of whole-body irradiation by penetrating gamma-rays or fast neutrons. The incentive has been obvious: the very large industrial hazard during the war-time development of the atom bomb, and afterwards the increasingly widespread risk associated with the remarkable development of atomic energy as a source of industrial power and of a unique series of military weapons. Chronic toxicity experiments in the strict sense must cover the whole life-span of the experimental animal and thus take years to carry out, even with the relatively short-lived laboratory mouse. The results of war-time work in the United States have become generally accessible in the past few years[1–5] and work carried out in this laboratory is just beginning to be published[6]. A brief survey of the experimental results relating to shortening of the life-span may provide a few facts in a field of current general interest and perhaps raise the academic question of how the results of chronic toxicity experiments, as such, may be generalized—a question which needs an answer before they may be used to help solve the practical problem of setting safe limits to the environmental exposure of man to irradiation.

Experimental Methods

Daily irradiation has been given to animals in a variety of ways, for details of which the original reports should be consulted[1–7]. The more important experimental features are summarized in Table 1. There were two important differences between the experimental arrangements of Henshaw *et al.*[3] and Evans[2] on one hand, and those of Lorenz *et al.*[4] and of this laboratory on the other. In the first two sets of experiments, the animals had to be transferred individually each day from their living cages to the irradiation boxes and back again, and each daily dose of radiation was given in a few minutes. In the second two sets of experiments, the animals were irradiated in their living cages, undisturbed by additional handling with its accompanying traumatic effects, and the daily dose of radiation was spread over 8–24 hr. In general, the experimental animals were examined daily and the time of death noted. Post-mortem examinations to determine tumour incidence and the cause of death were usually made, but the experimental reports differ very greatly in the detail with which these findings are given. For this reason, and since shortening of life-span is often considered the most sensitive experimental index of the toxicity of chronic irradiation, survival-time is the only experimental end-point considered here.

Results and their Interpretation

By chronic irradiation is meant daily irradiation 5, 6 or 7 days a week at dose-levels which allow survival for at least six months. All the experiments on chronic irradiation for the duration of life which have ever been carried out, so far as is known, are referred to in Table 1 and, where possible, shown in Fig. 1. The duration of life of an irradiated group of animals has been expressed as a proportion of its corresponding control and plotted against the weekly dose of radiation on a logarithmic scale. The results from this laboratory are shown in black symbols. They provide the first direct experimental comparison between gamma-rays and fast neutrons for chronic irradiation, where the dose of fast neutrons was measured in terms of energy absorbed in tissue. The relative biological efficiency factor for the fast neutrons used as compared with cobalt gamma-rays was 13.

This factor has been applied to the other two fast-neutron experiments, where the fast-neutron dose was measured in arbitrary units and where a somewhat uncertain conversion factor (Table 1) has to be used for estimating the tissue dose. In this way the results of all the experiments with fast neutrons as well as those with gamma-rays from other laboratories have been plotted, using open symbols, together with our own results in Fig. 1. The agreement, when mice were used as experimental animals, is remarkable, and suggests, in spite of the various uncertainties in the comparisons, that chronic irradiation shortens the life of mice in a reproducible manner.

It should be noted that there are eight experimental points at weekly doses of less than 10 r. or its equivalent in neutrons, and that the duration of life

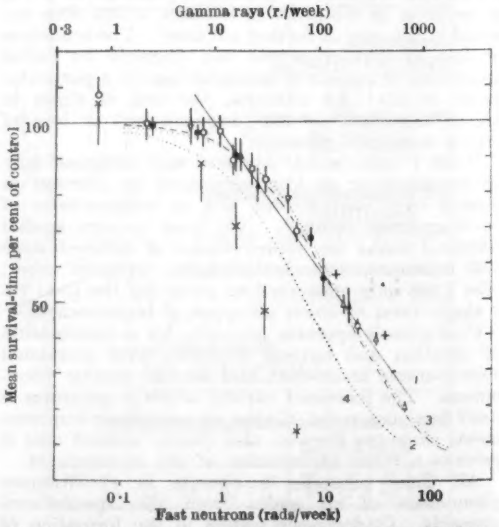

Fig. 1. Mean survival-time (per cent of control) and weekly dose of radiation (logarithmic scale)
The symbols are given in Table 1. The curves are numbered as in the text, where they are discussed. The gamma and neutron scales are in the ratio 13 : 1 (see text)

Table 1

Ref.	Source and type of irradiation G = gamma-rays N = fast neutrons	Unit of dose (conversion factor to rads)	Details of irradiation exposure: Days/week	Duration of daily dose	Symbol use l in Fig. 1	Experimental animal: Mouse strain	Age at start of irradiation (days)	Control life-span from start of irradiation) (days)	No. of animals used	Method of reporting survival-time
Henshaw *et al.* (ref. 3)	^{182}Ta G	r.	6	minutes	▽	CF_1 (females only)	?	440	820	Median*
	Graphite reactor N	r. (2·0)	6	minutes	△					
Evans (ref. 2)	Cyclotron N	N (2·5)	5	minutes	□	CF_1	28–42	420	500	Median†
						Swiss		475		
Lorenz *et al.* (ref. 4)	Radium G	r.	7	8 hr.	○	LAF_1	52–85	703	240	Mean
Neary *et al.* I (ref. 6)	Graphite reactor N	rad	6–7	16–24 hr.	■	*CBA*	75–95	780	500	Mean‡
Neary *et al.* II (ref. 13)	Graphite reactor N	rad	7	16–24 hr.	▲	*CBA*	45–75	818	320	Mean‡
	^{60}Co G	r.	7	24 hr.	●	*CBA*				
Thompson *et al.* (ref. 16)	^{60}Co G	r.	7	24 hr.	+	Rats (Sprague-Dawley, females only)	90–120	585	42+	Mean
Lorenz *et al.* (ref. 4)	Radium G	r.	7	8 hr.	×	Guinea pigs (hybrid)	137–196	1,372	112	Mean

* Mean survival times calculated from data provided by Hollaender and Stapleton (1948, personal communication) have been used in Fig. 1.

† Mean survival-time of the two strains combined were also reported and have been used in Fig. 1 because standard errors were also given. However, irradiation stopped when 8–30 per cent of an experimental group was still alive, so that the mean survival times include variable proportions of radiation-free time.

‡ There were real sex differences in control life-span and possibly also in the effects of irradiation. The data have been pooled to make them comparable with those of the other authors.

The data of Henshaw (ref. 7) have not been included because the mean life-span of his controls was less than a year. The data of Boche (ref. 1) have not been included for a variety of reasons: his monkeys had tuberculosis, his mice salmonellosis; the dogs and rabbits were irradiated in small numbers and irradiation stopped after two years, long before the end of the natural life-span; irradiation of the rats also ceased after two years when 16–36 per cent of the lower level and control groups were still alive and were killed, which prevents estimation of mean survival-times.

Evans's X-ray data (ref. 2) have not been included because mean survival-times were not given. The control life-span was not given by Hagen and Simmons (ref. 5). In each of Sacher's (ref. 5) and Mole's (ref. 11) experiments with daily X-irradiation of mice, one experimental group survived about seven months; they are omitted because no groups surviving longer are available and because the relative biological efficiency for X- to gamma-rays for chronic irradiation is not known.

in none of these experimental groups was significantly different ($P \geqslant 0{\cdot}05$) from its control.

The experimental results have been put down as they were obtained. More sophisticated analyses of some of these results have been made elsewhere[1,5,6,9,10]. The purpose of such analyses has usually been to find some regularity in the results which would allow extrapolations to daily doses smaller than, and to species other than, those used experimentally.

Curve Fitting

Three curves have been fitted to the mouse data and are shown in Fig. 1.

(1) The straight line which provided the relative biological efficiency factor of 13 from our second experiment (Neary *et al.*, II, Table 1) is clearly a good fit to its results, and is also reasonably close to the only experimental group in our first experiment with a markedly decreased survival-time. The simplest interpretation of such a linear relation is that there is a threshold of between 1 and 2 r. daily below which no shortening of a mouse's life will be produced by daily irradiation. This may be considered confirmed by the repeated experimental failure to find a demonstrable shortening at weekly doses of less than 10 r. (see above). Considering the nature of the data, it would be difficult to have a clearer experimental demonstration of the existence of a threshold.

(2) The biologist, almost as a reflex, attempts to fit a Gaussian curve to quantitative data. Such a curve is shown as a dashed line in Fig. 1, and clearly fits all the experimental data very well. The meaning of the fit at weekly doses of less than 10 r., where none of the points differs significantly from the base line, is less clear.

(3) Boche (1946, 1954)[1] suggested that shortening of life-span was proportional to the total accumulated dose,

$$t - t_0 = kdt$$

where t and t_0 were the mean life-spans of irradiated and control animals, d was the daily dose of radiation and k was a constant. This curve ($k = -0{\cdot}04$ for gamma-rays) is shown in Fig. 1 as a dotted line, which also fits all the experimental points very well.

Curves 1 and 2 are empirical; curve 3 has some claim to a theoretical basis, the idea that the bigger the total dose of radiation the bigger the effect, that is, the shorter the mean life-span. For daily exposures which kill in less than six months, however, the converse is found to be true[4,9,11]. This is not as paradoxical as it may seem, once the importance of recovery processes is appreciated; but it makes data on the effects of high daily doses (on shortening of life by much more than 50 per cent) of little value in helping to decide which is the best of several curves, each purporting to describe the effects of low daily doses.

Curves 2 and 3 are clearly so close together that over the experimentally determined range they cannot be distinguished. (The possibility that this algebraic similarity has a much wider biological significance is being investigated.) Each curve appears to fit all the points better than the straight line of curve 1, but this may be a spurious consequence of experimental uncertainties. In two experiments the exact conversion factor from arbitrary units of

fast neutrons to rads is unknown (see above) and factors numerically different from those used (Table 1) but just as plausible (see literature) would make the fit look less good. There seems to be no intrinsic reason why different mouse strains *should* behave identically, and the curvilinear arrangement of the experimental points may merely reflect differences of strain and of dose.

Each of the second two formulations indicates that there is no absolute threshold for shortening of life by chronic irradiation. The apparent threshold suggested by curve 1 may be thought of either as an absolute or as an effective threshold, depending on whether shortening of life is considered in proportional or absolute terms. If time is necessary for the effects of daily irradiation to show themselves, and if this time is longer the lower the daily dose, then an effective threshold *must* be reached at a dose-level which takes longer than the life-span to produce its effect. If so, each species would be expected to have its own threshold, and the longer the natural life-span the lower this would be. The only relevant experimental data are those of Lorenz *et al.*[4] on chronic irradiation of guinea pigs and these are included in Fig. 1. The effect of 1·1 r. daily was possibly greater than in mice (though still not significantly different from its control) and the apparent threshold possibly a little less. The difference in life-span between mice and guinea pigs is probably not large enough to decide the point, and in any event there are no confirmatory data for guinea pigs as there are for mice.

The data for guinea pigs do show that species differences occur. Boche[1] suggested, on admittedly tenuous evidence, that the constant k (curve 3) is αt_0, where α is the same for all mammals. If this were true, the mouse data should not agree so well, since t_0 for the different mouse strains differed. If the mean mouse t_0 is 600 days, $\alpha = 7 \times 10^{-5}$ (rather different from Boche's own estimate), and this has been used to construct the theoretical curve for guinea pigs ($\alpha t_0 = -0{\cdot}09$, curve 4, Fig. 1); the fit to the experimental points is poor.

Nature of the Experimental Material

In any event, too much should not be read into the results because of the nature of the experimental material. First, the results have all been expressed in terms of mean survival-times. This is really a rather unsatisfactory parameter to use, as may be seen from Fig. 2, which illustrates the shape of the mortality curve of normal control female *CBA* mice. The shape of the human mortality curve in the more materially advanced human civilizations is similar, but that of mice with a high spontaneous incidence of leukæmia may be very different[6,12]. The mean survival-time and its statistics are markedly affected by the occasional early deaths and no great precision in mean survival-time can be expected. A small decrease in mean survival-time could occur either because of a small increase in the frequency of earlier deaths or because of a small reduction in life-span of the upper two quartiles. In fact, an analysis of cause of death in relation to duration of life is imperative in order to see whether irradiation decreases life-span by increasing the frequency of particular causes of death which kill earlier than the average, or merely by making all causes of death kill at an earlier age[6].

Second, the nature of a chronic toxicity experiment usually, if not invariably, makes it impossible to randomize treatments and to ensure that the only difference between experimental groups is the treatment being investigated. For example, if animals are arranged at different distances from a source of radiation, the animals will occupy different parts of a room for their whole lives and it will be impossible to be sure that environmental temperature, humidity, degree of air movement and other relevant factors possibly not even thought of, are exactly the same for each different dose-group. Thus the differences in, say, mean survival-time between different groups will be due to the differences in radiation-level plus any other relevant environmental differences. This is not just a theoretical point. Differences of the order of 5 per cent in the mean survival-time of female *CBA* mice have been found during the past few years not only between different 'lots' of controls but also between two sets of randomly chosen controls kept, so far as could be, in the same environment but some 20 ft. away from each other[6]. The apparent increase in survival-time at the lowest daily dose used by Lorenz *et al.*[4] (Fig. 1) may well be due to the fact that the animals at this dose-level were kept without air conditioning in a different room from all the other groups, including the controls. Such variability is to be expected by the biologist, but it should also enjoin caution in extrapolation of the results of analysis of intrinsically inexact data.

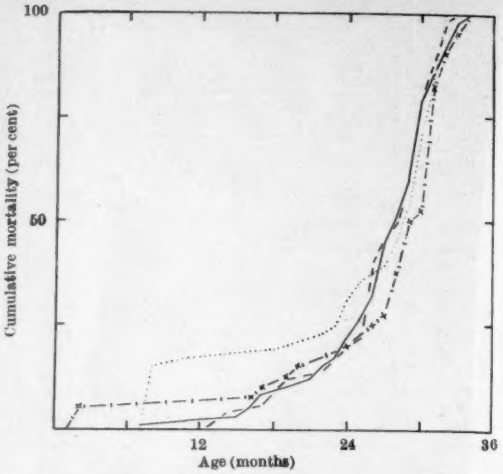

Fig. 2. Cumulative mortality of female *CBA* mice (four different control groups 1951–54). All times plotted from same starting age of 70 days

Replication on a sufficiently large scale, though often completely impractical, could overcome this particular difficulty. In fact, however, replication is almost completely lacking from the experiments listed in Table 1. The logic of experimentation is that experiments are repeated and give the same result. Yet with the exception of a still unfinished investigation[10], no one concerned with duration-of-life irradiation experiments has ever repeated his experiment even once—for which there are perhaps understandable reasons. The nearest to repetition so far has been the two experiments carried out in this laboratory[6,13], where although the same mouse strain was used the radiation doses were different. From this point of view the value of Fig. 1 is to demonstrate that an experiment has been done, that is, that the same result has been obtained several times over.

Lastly, it should be pointed out that in all the experiments considered here irradiation has been for the duration of life. This may not be the most appropriate experiment to carry out. Recent[6,14,15], as well as older[4,16], evidence has shown that, in some circumstances at least, not all radiation is of equal value, the first of a series of daily doses having proportionately greater effects in shortening life and inducing leukæmia than the later daily doses. This is presumably one aspect of the time factor; time is needed for the effects of irradiation to develop to the point where biological damage can be detected[11,14,17], and/or the reactivity of the biological object may change with age[11]. But if the phenomenon is true of weekly doses of less than 50 r., which has not yet been demonstrated, formulæ which give equal weight to each of a series of doses, as Boche's, cannot be properly extrapolated. Further, if at relatively high daily doses much of the radiation is wasted, so far as producing an effect is concerned[11], then an observed linearity of response against total dose (curve 3, Fig. 1) may imply a decreasing ability of radiation to harm as the daily dose decreased.

There has also been very little work yet on the problem of whether the effect of chronic irradiation is altered by changing the distribution in time of, say, a constant weekly dose. The data of Table 1 and Fig. 1 suggest that it matters little whether a daily dose is given in a few minutes or spread out over many hours; but other as yet uncompleted observations[14,17] suggest that the delayed effects of irradiation may depend as much on the way the irradiation is given as on the total dose. In these experiments there was no wasted radiation: on the contrary, as much time as possible was allowed for the full development of any damage that radiation may have caused. Such experiments may give a relation between shortening of life and dose of radiation very different from those shown in Fig. 1, and indeed this might well be anticipated by anyone aware of the normal complexity of biological phenomena. Dose–response curves should not be extrapolated without fully realizing the nature of the experimental material on which they are founded.

Possibilities of Extrapolation

It should first be emphasized how unusual it is to pay any attention to the ends of a biological dose–response curve. Normally, the aim of the biologist is to work in the middle ranges and, if irregularities appear at the ends, this is regarded as just to be expected, not necessarily deserving investigation.

The current maximum permissible level of radiation for occupational exposure of man, 0·3 r. weekly (Recommendations of the International Commission on Radiological Protection), is indicated in Fig. 1. Extrapolation suggests that this dose-level would shorten the lives of mice by nil, 0·02 or 0·2 per cent, depending on which of the three curves described earlier is taken to be correct. As already shown, the experimental data on chronic irradiation at low doses are not sufficiently exact to distinguish between the curves, and the adequacy of fit at high levels of irradiation seems quite irrelevant. Thus the value of any attempt at extrapolation must depend on whether there is some theoretical reason for preferring one mathematical form to another. When this question is settled, there is the additional problem of extrapolating from one species to another.

One principle of selection often used nowadays in general discussion on radiation as it affects mankind, and at first sight self-evidently sound, is to take the most pessimistic assumption suggested by experiment or theory for the relation between dose and effect. Lorenz[8] used a very similar criterion when discussing the effects of daily irradiation on the different tissues and organs of different species. He concluded that man should be considered to be as sensitive as that species of animal found experimentally to be the most sensitive. Clearly this is no absolute criterion: as the range of species examined is widened, the apparent sensitivity of man must decrease. A consistent use of this criterion would involve denying the possibility of chemotherapy, or of selective killing by pesticides. It does not seem realistic to maximize pessimism as a means of choosing the best dose–response curve.

The most plausible reason for thinking that species differences among mammals in their reactions to irradiation are likely to be smaller than in their reactions to chemical agents is that the penetration of radiation into cells is not affected by the series of permeability barriers which every chemical agent has to pass before reaching the site of its action[18]. The uniformity of the acutely killing dose for all mammals gives supporting evidence. However, the chronic toxicity of radiation would be expected to depend on a balance between the continuing damage produced by the radiation and the ability of the irradiated animal to keep pace with the damage by repair. The ability to repair and its rate must depend on many of the structural and metabolic features which distinguish strains and species, and, for this reason, strain and species differences in the dose–response curves for chronic irradiation might be expected. Some of the experimental facts can best be understood in this way[6].

An alternative view is to assume that the chronic toxicity of radiation is due to processes where repair of damage does not occur, like genetic mutation. It may then be plausibly argued that the genetic material of all mammals is very similar, both physically and chemically, and that therefore dose–response curves will in general be the same for all species. Such a view would suggest that damage should be proportional to total dose, as in Boche's formula (curve 3, Fig. 1), and would be consistent with the somatic mutation theory of carcinogenesis and the fact of carcinogenesis by ionizing radiation. But there are difficulties in the way of equating damage and total dose, as already suggested, and really very little evidence in support of the mutation theory of carcinogenesis. The theory is an easy one to accept; but even with the most recent advances in technique its testing seems almost impossible to envisage. However, in the experimental animal there is no simple relation between carcinogenesis and dose of radiation, and for mouse leukæmia there is good evidence of the great importance of an indirect mechanism[19]. Moreover, the experimental evidence suggests that radiation shortens life apart from inducing cancer, and this is not easy to understand in terms of mutation.

If the results of animal experiments are to be carried over to man, there must either be very good evidence that all mammals behave alike, or sufficient human evidence of similarity with experimental animals to inspire confidence in the process of filling the human gaps from animal experience. It will at least be generally agreed that experimental dose–

response relations which cannot satisfactorily account for all experimental results are scarcely worth applying to the human case. In the absence of a satisfactory theory, it seems pointless to expend the enormous experimental effort required to define the relation between daily dose and life-span for mean survival-times of 95 per cent and more of the control: it is only in this region that extrapolation to man is of any particular interest.

I would like to thank my colleagues for allowing me to make use of unpublished material.

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LOW-TEMPERATURE PHYSICS IN THE U.S.S.R.

THE fourth Russian conference on low-temperature physics was held during July 2–8 at the Institute for Physical Problems of the Soviet Academy of Sciences in Moscow. Six physicists from Great Britain, Drs. R. G. Chambers, H. A. Hall and A. B. Pippard from Cambridge, and Drs. R. Berman, A. H. Cooke and the author from Oxford, were invited to this meeting as guests of the Academy and were the only foreign visitors.

Since this was the first meeting with Russian low-temperature workers, a brief enumeration of the Soviet cryogenic research centres may be of interest. There are two laboratories in Moscow, since, besides the Institute of the Academy, there is also a cryogenic research team under A. I. Shalnikov working at the University. Kapitza is the director of the Institute for Physical Problems, but in the past two years he has devoted himself to other than low-temperature research. Peshkov is the deputy director and Molkov is in charge of the section dealing with cryogenic engineering. Another section, in a separate building, carries out research into the electronic behaviour of metals and is in the charge of Alekseevsky. Also at the Institute there is a theoretical physics section of which Landau is the head and in which E. M. Lifshitz and Khalatnikov are senior members. Kharkov has an old-established cryogenic laboratory, with Lazarev as director and I. M. Lifshitz and Kaganov as theoreticians. Of more recent date is the low-temperature laboratory at Tiflis, which is under the direction of Andronikashvili. There is another new cryogenic laboratory in Sverdlovsk.

At the conference, papers and discussions, except those by the visitors, were given in Russian; but the visitors were supplied with headphones and a simultaneous translation was provided for them. For this translation Kapitza had obtained the services of Mr. Boris Belitzky, of Moscow Radio, who is a physico-chemist by training. Mr. Belitzky's truly amazing gift for rendering not only the lectures but also the—often heated—arguments accurately into faultless English made all the difference to the success of the meetings so far as we were concerned. He clearly lived the part of the speakers, and the visitors came to forget that the voice they heard was not that of the lecturer. Judging from adverse reports on similar meetings, the value of a really good interpreter cannot be assessed too highly at a conference of this type. We were able to follow the proceedings so completely that a whole week of lectures in Russian never became tedious.

The following brief summary of the papers presented at the conference cannot give any details of the experimental results or the theoretical investigations which were reported. Its object is to give an idea of the type of research in progress in the U.S.S.R. and to provide a list of the work done by the various authors so that interested readers can apply to them for reprints or further information*.

After an opening address by Kapitza, the conference began with a series of papers on liquid helium which occupied two sessions. Much of the work was concerned with the light isotope and with isotope mixtures. The viscosity of pure liquid helium-3 has been measured between 0·35° and 3·2° K. by Miss K. N. Zinovieva, who works in Peshkov's laboratory. She reported a sharp rise of the viscosity with falling temperature below 1° K. which, however, at the lowest measured temperature has not yet reached the theoretically predicted value of T^{-2}. Above 1° K. a slight drop in viscosity with rising temperature is in qualitative agreement with the theory of Abrikosov and Khalatnikov. It is interesting to note that she also found a slight drop in the viscosity of helium-4 above 2·6° K. instead of the slight rise with temperature reported by Bowers and the present author seven years ago. She ascribed (correctly) this discrepancy to the omission of a correction in our work. Miss T. P. Ptukha, working in the same laboratory, presented measurements of the density of mixtures of liquid helium-3 and helium-4 and deduced lambda temperatures for four concentrations from the break in the density curves. The Kharkov workers, B. N. Eselson, A. D. Shvets and R. A.

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Bablidze, have determined the lambda points of mixtures of helium-3 and helium-4 by the onset of film flow. An interesting theoretical investigation into the phenomena to be expected when first or second sound passes through a phase boundary between solutions of helium-3 and helium-4 has been made by R. G. Arkhipov and I. M. Khalatnikov. They gave the equations for reflexion, refraction and conversion from one type of sound into the other for the cases when both or only one of the phases are superfluid. In another theoretical paper, by L. P. Gorkov and L. P. Pitaevsky, the scattering of light in helium-3–helium-4 solutions was calculated, and it was shown that whereas pure helium-4 should exhibit only four lines, the spectrum of the solutions should contain five. A theory of the momentum distribution of interacting particles in Fermi systems was proposed by A. B. Migdal.

Except for some theoretical considerations of the helium film by R. G. Arkhipov and a report on normal viscosity and the onset of friction given by the present author, the rest of the work on helium was concerned with rotation experiments. Both H. A. Hall as well as D. S. Zakadze and E. L. Andronikashvili described oscillation experiments in rotating liquid helium. An investigation into the existence of discontinuity surfaces in moving liquid helium, postulated by Ginsburg, has been made by G. A. Gamzemlidze, who concludes that the surface energy of such discontinuities cannot exceed 10^{-9} erg/cm.^{2}. A theoretical attempt to estimate the relaxation time of vortices in rotating helium has been made by Y. G. Mamaladze who, like the previous author, works in Tiflis. E. M. Lifshitz and L. P. Pitaevsky discussed the scatter of rotons on the vortex lines in rotating helium and have postulated that a strong interaction must exist between these two if the theory is to explain the experimental observations.

A good deal of the work on superconductivity dealt with the boundary conditions between the two states, using the Landau–Ginsburg theory as the guiding principle. The corrections due to relativistic magnetic interactions were taken into account by V. L. Ginsburg and L. V. Keldysh. The first author also presented a paper evaluating the spread of magnetic field around the equilibrium value in which supercooling and overheating might be expected. Closely connected with these considerations is an interesting theory of A. A. Abrikosov on the behaviour of superconductive alloys. The well-known fact of the gradual magnetic penetration over an interval of field values is considered as a basic property of the alloy rather than due to macroscopic inhomogeneities. Measurements of the surface tension between the superconductive and the normal state in very pure tin single crystals were reported by Y. V. Sharvin, who found that the observed values are of the same order of magnitude but somewhat smaller than the values predicted by the Landau–Ginsburg theory. The conditions for the possible occurrence of superconductivity in bulk samples of ferromagnetic materials were discussed by G. F. Zharkov. He suggests the use of flat ellipsoids of rotation with a large demagnetization factor as most suitable for the detection of superconductivity.

The method of thermal waves for determinations of heat conductivities and specific heats has been employed by N. V. Zavaritsky, who investigated tin, aluminium and zinc down to 0·15° K. Dependence of the thermal conduction on the shape and surface conditions of the specimens was observed as well as an exponential rise with temperature of the specific heat in all three metals. A survey of heat conductivity work on superconductors, carried out in Oxford, was given by the present author. B. G. Lazarev and A. I. Sudovtsov reported measurements of the volume change occurring in tin and lead when superconductivity is established in a magnetic field, and of the discontinuity of the expansion coefficient at this transition. They found good thermodynamical agreement with the data for the effect of pressure on the transition of these metals. The same authors, together with A. P. Smirnov, have observed superconductivity in thin beryllium films which had been deposited on a helium-cooled surface, the transition occurring at about 8° K. The depth of penetration in a very pure cadmium sample has been determined by N. S. Khaikin, using a high-frequency method. The measurements were carried down to about 0·2° K., where a value of $1{\cdot}5 \times 10^{-5}$ cm. (much smaller than the anomalously high value given by Hein and Steele) was found. Similar measurements on films of tin and mercury have been made by Miss L. A. Prozorova, who concludes that the dependence upon temperature of the depth of penetration is in good agreement with the Landau–Ginsburg theory. Further theoretical considerations based on these results were reported by B. M. Bolotovsky.

A general survey of the possibilities of Fermi surface investigations in metals was given by I. M. Lifshitz, and this was followed by a paper by A. B. Pippard on the Fermi surface in copper. Two separate sets of measurements on the influence of pressure on the de Haas – van Alphen effect were reported. One, by N. B. Brandt and V. A. Ventzel, dealt with a detailed investigation on bismuth; while in the work of B. I. Verkin, I. M. Dmitrenko and B. G. Lazarev, zinc and tin were studied as well. The amplitude of oscillations of susceptibility is decreased in all cases by the application of pressure. Another approach to the determination of the Fermi surface, through cyclotron resonance in metals, was discussed theoretically by M. Y. Azbel and E. A. Kaner, who have amplified their former work by calculating the surface impedance as a function of field and temperature over a wide range of fields. Experimental data of cyclotron resonance in tin were presented by P. A. Bezugly and A. A. Galkin, and a paper on cyclotron resonance in bismuth was given by R. G. Chambers. The theory of the anomalous skin effect in metals in the infra-red was discussed by R. N. Gurzhi.

The resistance minimum in gold has been investigated in some detail by N. E. Alekseevsky and Y. P. Gaidukov who, from magneto-resistance observations and measurements of the Hall effect and of the magnetic properties, have formed the opinion that at about 8 k.oersteds a change in the metal takes place which, however, requires further elucidation. The galvano-magnetic properties of bismuth, tin and aluminium have been studied by N. E. Alekseevsky, N. B. Brandt and T. I. Kostina. P. G. Strelkov and Miss I. N. Kalinkina have measured the specific heat of bismuth from 0·3° to 4·4° K. and found that it is composed of a cubic term and a very small linear term. The effect of isotopes on the heat conduction of dielectric crystals was discussed by R. Berman. O. V. Klyavin and A. V. Stepanov gave data for the mechanical properties of a number of metals at liquid-helium temperatures.

The last session of the conference dealt with magnetic phenomena. The energy spectrum of antiferromagnetics near the ground-state was discussed by M. I. Kaganov and V. M. Zukernik. A. H. Cooke reported on antiferromagnetism in some iridium compounds, and antiferromagnetism in anhydrous sulphates and carbonates of the transition metals was described by A. S. Borovik-Romanov, N. M. Kreines and Miss M. P. Orlova. The work of the latter authors has led I. Dzyaloshinsky to a theory of weak ferromagnetism in antiferromagnetic crystals which, based on Landau's ideas, explains the effect as due to relativistic spin-lattice and magnetic dipole interactions. The propagation of sound in ferromagnetics was considered by A. I. Akhiezer, B. G. Baryakhtar and S. B. Peletminsky, who postulate that it should be possible to excite magnetic waves by means of sound, and that under certain stated conditions this should be an easily noticed effect.

Two afternoons of the conference were devoted to visits to the cryogenic laboratories at the Institute for Physical Problems and at the University of Moscow. An interesting historical exhibit at the former was the first turbine used by Kapitza in the liquefaction of air. Among the various research installations on show, a helium-3 cryostat in Peshkov's laboratory was particularly impressive. In this ingenious device five litres (at N.T.P.) of pure helium-3, which forms the working substance, is circulated constantly. The gas condenses under pressure in helium-4 and is then expanded. Temperatures down to 0·3° K. can be obtained in this way and temperatures down to 0·5° K. can be maintained constant for indefinite periods. Peshkov and Miss Zinovieva showed us a photograph, taken in this apparatus, in which the separation of the two liquid phases of helium-3–helium-4 mixtures was clearly visible. At the University of Moscow we saw besides the research laboratories the cryogenic teaching course, where undergraduates were busy with their ten experiments involving liquid helium. One of them was just carrying out an adiabatic demagnetization.

The members of the conference came to know each other well on a whole day's boat trip arranged by Kapitza. The hospitality of the Academy and the friendly welcome of our Russian colleagues made our stay in Moscow a very pleasant one. The visitors expressed the hope that they will be able to induce their hosts soon to come to Britain for a return visit.

K. Mendelssohn

OBITUARIES

Prof. H. O. Wieland, For.Mem.R.S.

To-day the manufacture of steroids for medical purposes is an industry. Hundreds of scientists in academic and industrial life are engaged in research on these substances. All this activity properly began in Germany, and its modern phase really dates from the work of A. Windaus on cholesterol and other sterols, which began in 1903, and that of Heinrich Wieland, whose paper (with F. J. Weil) on the preparation of the 'stem' bile acid, cholanic acid, $C_{24}H_{40}O_2$, was published in 1912 (*Hoppe-Seyl. Z.*, **80**, 287). Until 1949, fundamental discoveries, mainly about the occurrence in Nature and the chemical structures of bile acids, toad poisons and some sterols and alkaloids were published continually from Wieland's laboratories. These contributions have been reviewed, for example, by L. F. Fieser and M. Fieser ("Natural Products related to Phenanthrene". Reinhold, New York).

The award of a Nobel Prize in Chemistry to Wieland in 1927 and to Windaus in 1928 anticipated our full appreciation of the importance of their work and of the use which would be made of it.

When O. Rosenheim and H. King in 1932 (*Chem. and Indust.*, **51**, 464) put forward their revolutionary chrysene structure for sterols and bile acids, Wieland and E. Dane (*Hoppe-Seyl. Z.*, **210**, 268; 1932) were quick to see its advantages and almost at once proposed the formula now accepted. Wieland and his colleagues went on to show how this formula explained observations which they and the school of Windaus had collected for nearly thirty years.

At that time, the structures of the crystalline œstrogenic hormones which had been isolated independently by E. A. Doisy, C. D. Veler and S. Thayer (œstrone, 1929), G. F. Marrian (œstriol, 1930) and A. Butenandt (œstrone, 1929) from human pregnancy urine were still unknown; a leap in our understanding followed the demonstrations that these hormones were related to cholesterol and the bile acids. The androgens, progesterone and the adrenal cortex hormones soon appeared in the same pattern. Antirachitic vitamins D were derived from ergosterol and, later, cholesterol. When Wieland and Dane (*Hoppe-Seyl. Z.*, **219**, 240; 1933) and J. W. Cook and his associates, at the same time, converted a bile acid (deoxycholic) into a powerfully carcinogenic hydrocarbon (methylcholanthrene), the foundations of much present-day interest in steroids had been laid.

Many new sterols and bile acids have since been discovered, and we now know that the heart poisons of toads and in certain plants, some plant sapogenins and alkaloids, as well as triterpenes of diverse origin, can be regarded as steroids. We have become adept at converting common sterols and sapogenins like cholesterol, diosgenin and hecogenin into known hormones and D vitamins, as well as into other substances with many kinds of physiological activity. The principal steroids have been totally synthesized. Many chemical reactions of these substances can now be interpreted in terms of electronic theory. We have begun to understand how sterols are synthesized *in vivo* from simple substances like acetate and how sterols can give rise to bile acids and hormones. Studies of the metabolism of such hormones in the living body are far advanced. We grope towards comprehension of the significance of sterols themselves in living cells and of the means by which steroid hormones may affect cellular chemical reactions.

An immense amount of work remains to be done. No doubt fundamental changes in our appreciation of vital processes must follow or precede more complete understanding of the natural significance of steroids.

It is scarcely too much to say that without the work of Windaus, Wieland and their colleagues, no such achievements would have been possible, and no such exciting prospects would now lie before us. Prof. Windaus is still alive, but Prof. Wieland died recently at the age of ninety.

G. A. D. HASLEWOOD

Prof. William Rowan

DR. WILLIAM ROWAN, emeritus professor of zoology in the University of Alberta, died at Edmonton, Alberta, on June 30 at the age of sixty-five. Born in Basle, Switzerland, on July 29, 1891, he emigrated to Canada in 1908. For a time he worked on a ranch near Dorothy, Alberta, and, in later life, often spoke with some pride of his prowess as a cowboy. But while this outdoor life fostered a latent interest in biology, young Rowan's inquiring mind found this mode of development too slow and he soon went to Britain to complete his formal education. This was interrupted by the First World War. Rowan enlisted as a private in August 1914, and served with the London Scottish Regiment until 1916. He then returned to his studies and graduated in the following year from University College, London, with the B.Sc. degree. His artistic talents began to show at this time. A period of study at the Slade School of Art developed skills in drawing and sculpture. In future years his knowledge of animal life and a fine æsthetic sense combined to produce models and sketches of birds and mammals notable in particular for their flow of line. He studied music and became an accomplished pianist. So strong was his love of music that he thought seriously of turning to the concert platform. Throughout his life he was never quite certain that he had made the right choice in following the path of science.

After the War, Rowan returned to Canada to lecture in zoology for a year at the University of Manitoba. In 1920 he went to the University of Alberta at Edmonton, where in the following year, with the rank of associate professor, he founded the Department of Zoology. He remained as head of this Department until his retirement in 1956. He was a popular and stimulating lecturer, fond of using startling statements to shake students from their mental lethargy.

Of the various branches of zoology, Rowan had always shown a preference for ornithology and now, established in a university community, he proceeded to investigate some of the problems which had intrigued him for years. One of these was the migration of birds. He postulated that there is an inherent migratory habit in some species of birds which is impelled by a hormone the production of which is controlled by the environmental factor of changing day-length. Working at times under great difficulties, he kept canaries and juncoes in cages in his own garden and produced the first experimental evidence that fluctuations in day-length cause a cyclical development and regression of the gonads of birds. This was the first step in proving the hypothesis that photoperiodism is a controlling factor in the phenomenon of avian migration. In further experiments he attempted to make juncoes sedentary during the normal migratory period and to reverse the usual direction of migration in others. The results of this work were presented in a thesis for the D.Sc. degree of the University of London in 1929. In that year, also, Rowan was elected a Fellow of the Zoological Society of London.

Results of the experiments with juncoes were not conclusive. This, Rowan felt, was largely due to the small size of the bird, with consequent difficulty in recognizing and recapturing the experimental birds. He therefore determined to repeat the experiments using crows as subjects. The publicity attendant upon capture, treatment, release and recapture of these birds put his work and his name upon the tongues of laymen throughout North America and undoubtedly assisted greatly in popularizing science. His book, "The Riddle of Migration", published in 1931, included the results of this work and brought him international recognition. It also marked the beginning of a decline of interest in the problem of migration. His scientific efforts were henceforth largely directed toward a study of the cyclical fluctuation in numbers of certain species of North American birds and mammals, a study which he felt was essential to a complete understanding of the problems of conservation on the continent. In 1946 he was awarded the Flavelle Medal of the Royal Society of Canada in recognition of his investigations. He was working on various aspects of animal cycles at the time of his death.

Always a firm believer in an oligarchy of the intelligentsia, Rowan looked upon the events following the Second World War as conclusive evidence of the depravity of democracy. He was convinced that mankind is heading towards total self-destruction and that it can be saved only by placing itself under the guidance of an intellectual few; after the development and use of the atom bomb his prognostications became even more gloomy.

Rowan's influence in the field of experimental ornithology will long be felt. The world-wide circle of friends with whom he discussed favourite subjects over coffee and cigarettes will feel not only a sense of great personal loss but also sincere regret at the passing of a large heart and a fine mind.

W. RAY SALT

Dr. Gerald Roche Lynch, O.B.E.

THE death of Roche Lynch at the age of sixty-eight on July 3, at his home in Slough three years after retirement, ended a long period of distinguished service to medical science. It also erased the name of the last of a group of remarkable men, including Pepper, Luff, Spilsbury and Willcox, who for thirty years in the first half of this century had been the mainstay of the Home Office in scientific crime investigation in England and Wales.

Roche Lynch, whose father was a Kensington medical practitioner, was educated at St. Paul's School and entered St. Mary's Hospital, London, in 1905, gaining an entrance scholarship in science and, a year later, the prize in chemistry. This was undoubtedly the turning point in his career, for while still studying medicine he also trained in chemistry under H. E. Armstrong at the City and Guilds Institute and demonstrated in the subject at his own medical school. The latter appointment brought him under the influence of William Willcox, who then lectured in chemistry, and together they were drawn into the practice of Pepper and Luff, no small part of which was medico-legal. Qualifying in medicine in 1913, Roche Lynch served in the Royal Navy until 1919, when he was appointed O.B.E. Returning

to St. Mary's as senior demonstrator in pathological chemistry, he was made head of the Department in 1926 and continued so until he retired in 1954. Willcox had him appointed junior to him as Home Office analyst in 1924 and he succeeded to the senior appointment four years later.

Roche Lynch was undoubtedly happiest at work in his somewhat untidy-looking laboratory at St. Mary's where, surrounded by the stalagmitic accumulation of what seems years of specimens, he never failed to find room for another 'most interesting case', to display a penetrating grasp of its nature and to keep orderly hand-written notes which were good enough to survive defending counsel's scrutiny on many an occasion. It was from this laboratory that he made literally thousands of appearances in coroners' and assize courts between 1924 and 1954 to give his findings in evidence. In this he was superb, for he had mastered the art of extraction without loss of accuracy, and, though precise, he gave sufficient hint of thoroughness of method and of painstaking laboratory care to deter defending counsel from pressing him far. He was a fine analytical chemist of the older school whose work in cases like that of Duff and Sydney, Violette Kaye, Vera Page and Sidney Fox was so sound that it acquired a remarkable reputation and gained him the respect of both the Bench and the Bar. Affection he did not encourage, and the Savage Club was virtually his sole excursion into social life.

He examined for Branch *E* for the fellowship of the Royal Institute of Chemistry—of which he was president during 1946–49, and he also examined for the London M.B. and the D.P.H. He became president of what is now the Society for Analytical Chemistry in 1936–37, of the Medico-Legal Society in 1939–41, and master of the Society of Apothecaries in 1950–1, filling these distinguished posts with a strong practical authority and with disarming humility. He was both liked and respected, and his long era of service to medicine and science, to the field of chemistry—in which he wrote many papers—and to university life in London will mark him as one of the distinguished figures of twentieth century medical science.

Keith Simpson

NEWS and VIEWS

Civil Engineering in Trinity College, Dublin: Prof. John Purser

Prof. John Purser, a member of a family whose name has been synonymous with that of Trinity College for many years, will retire from the chair of civil engineering in the University of Dublin on September 21. Prof. Purser graduated in engineering in Dublin in 1907, and decided to make his career as a teacher. He joined the Navy in the First World War, serving first in motor torpedo boats and later with a team working on paravanes. After the War he taught in the City and Guilds College in London, and later in the University of Birmingham. His research interests were the deterioration of structures in sea water, and the flow of rivers. He returned to Dublin in 1933, and devoted himself to teaching and to guiding the growth of the School of Engineering. After the Second World War, the premises of the School were reconstructed and enlarged, and a fourth year has recently been added to the course. The School is for civil engineering only, and Prof. Purser has had the satisfaction of seeing his graduates sought for by works departments all over the world. He has given freely of his time to the general affairs of the College, to the Institution of Civil Engineers in Ireland and to the Commission of Irish Lights, and all these bodies look forward to continuing to draw on his experience and advice.

Prof. W. Wright

Dr. W. Wright, who is succeeding Prof. John Purser, has been senior lecturer and head of the Civil Engineering Department in the University of Southampton since 1954. His main fields of interest have been structural engineering and hydraulics. In structures he has been particularly concerned with applications of the relaxation method. In hydraulics he will be remembered at Southampton for the design and commissioning of a large tidal model of the Solent and Southampton Water. Because of the need for two separate tidal generators at the two entrances to the Solent, this model proved exceptionally difficult to bring under control, so that the complex local tides could be correctly reproduced. Dr. Wright was successful in overcoming these difficulties in the short time available to provide the necessary information for a proposed dredging project.

Nuclear Physics at Oxford: Prof. D. H. Wilkinson, F.R.S.

Dr. D. H. Wilkinson has been appointed to the professorship of nuclear physics (established with the late Sir Francis Simon's support after he had succeeded Lord Cherwell as Dr. Lee's professor of experimental philosophy) at the Clarendon Laboratory, Oxford. Dr. Wilkinson graduated at Cambridge in 1943 and became a Fellow of Jesus College in 1945. For a short time he worked with the first heavy-water reactor at Chalk River in Canada; however, in the course of this or his previous work he had become exposed to more radiation than was good for him, and back in Cambridge after the War he accepted, for a time, a medical injunction to keep away from penetrating radiations and hence from nuclear physics. It was during that time that he started to work on how homing birds find their way, and he made some valuable contributions by exploding some untenable hypotheses and by inventing a simple radioactive clock for measuring the time a bird spends on the wing (*Nature*, 161, 997; 1948, and 165, 188; 1950). Fortunately, his health fully recovered and soon he was back in nuclear physics. His numerous contributions range from the development of ingenious new measuring techniques to the

skilful exploitation of new theoretical concepts (such as isotopic spin); he was elected a Fellow of the Royal Society in 1956. More recently, after a year's stay in the United States, he turned to the exploration of the strange particles which abound near particle accelerators of the highest energy. To his new appointment he brings a lively and versatile mind, a wide knowledge of physics, a gift for clear and vivid presentation and a subtle sense of humour.

Chemical Engineering at Birmingham : Prof. S. R. M. Ellis

Dr. S. R. M. Ellis, who has been appointed to the second chair in the Chemical Engineering Department of the University of Birmingham, had a wide industrial experience before becoming interested in university teaching. He is forty-four years of age and was born in New Zealand and studied in Auckland University, where he graduated and obtained the degree of master of science. He had extensive experience in the fuel industry in New Zealand before coming to Britain in 1937. Initially he was connected with the Low-Temperature Carbonization Oil Refinery at Bolsover, as chemical engineer on both the operating and design sides. He then became a senior chemical engineer to a chemical plant manufacturer, designing and fabricating complete chemical plants for the chemical industry. He joined the Chemical Engineering Department at the University of Birmingham in 1947, was appointed senior lecturer in 1949 and reader in 1955. He has been closely associated with the Institution of Chemical Engineers, Midlands Branch, as honorary secretary and has served on a number of educational committees. He has played a very active part in the teaching work of the University and in research activities, centred largely on distillation work, including vapour-liquid equilibria over a range of pressures and determinations and method of operation of various forms of distillation plate columns.

Grants for Learned Journals : Dr. F. V. Morley

The joint committee of the Royal Society and the Nuffield Foundation which administers grants made available by that Foundation to assist learned scientific journals has appointed Dr. F. V. Morley as liaison officer. The committee, presided over by Sir Edward Salisbury, has already made a number of grants to aid journals which are concerned in primary subjects of research. Dr. Morley's father, Frank Morley, was a Cambridge mathematician who (following a distinguished pattern which had been set before his time by J. J. Sylvester) became head of the mathematical department of the Johns Hopkins University, and from an early age the son was familiar with and interested in the problems attendant on the production and distribution of the *American Journal of Mathematics*. Writing, publishing, journalism, and respect for all branches of research were much in the blood of the Morley family of Baltimore. The eldest son, Christopher Morley, was a publisher before he became well known as a writer; the second son, Felix, became editor of *The Washington Post*, and after that president of Haverford College; the third son is the present appointee of Sir Edward Salisbury's committee.

All three of the Morley brothers were Rhodes Scholars at New College, Oxford. F. V. Morley took his D.Phil. at Oxford in mathematics, but instead of returning to teach in the United States he married and settled in England. He was on the staff of *The Times*, but left to become London manager for the Century Company of New York, and afterwards, in 1929, was one of the founding directors of Faber and Faber. He shared a room with Mr. T. S. Eliot and, as he says, shared in the headaches of publishing *The Criterion*. In 1939 he was invited to New York to become a director, and afterwards vice-president, of Harcourt, Brace and Co. In 1947 Mr. Morley (for outside academic circles he has never used his academic title) returned to London. In 1949 he became a director of Eyre and Spottiswoode (Publishers), Ltd., and of E. and F. N. Spon, Ltd. Dr. Morley retired from publishing a year ago, but it will be seen that he has had a wide and varied experience which the committee hopes may prove of practical value to those concerned with the production and distribution of learned and specialized journals. Perhaps the best possible assistance that can be rendered to the journals resides in increase of circulation, if that can be managed without any interference with editorial policy or any lowering of standards. Every promising effort to promote subscriptions for British journals, especially overseas, will certainly be welcomed. Inquiries may be addressed to Dr. F. V. Morley, Nuffield Lodge, Regent's Park, London, N.W.1.

Brazilian Society for the Progress of Science

The annual meeting of the Brazilian Society for the Progress of Science was held this year during July 8–13, for the first time in Rio de Janeiro. The president for this ninth meeting was Prof. Fritz Feigl, director of the Microchemical Laboratory of the Brazilian Department of Mineral Production. The inaugural session, which was attended by the Minister of Education, opened with the reading of a goodwill telegram to the Society from the British Association. During the following days, some 294 papers (contributed by 362 scientists) were presented in the various sections, and there were also nineteen sectional discussions. About two-thirds of the papers were presented in the biological or medical sciences. Of the remaining subjects, education, chemistry and physics were well represented. Evening lectures included talks by Prof. Walter Cruz on "The Social Importance of Science", by Sr. Oscar Niemeyer on the project for the construction of the new capital of Brazil, and by Mr. F. M. Beatty, the acting representative of the British Council in Brazil, on "The British Association". There were eighteen symposia and discussion meetings, including a discussion on the functions of Brazilian scientific institutions. The next annual meeting is planned to take place in São Paulo in July 1958.

Association of Island Marine Laboratories

The remarkable growth of interest in marine biology in the tropical western Atlantic was made apparent by a conference held during April 1957 at the marine laboratory of the University of Puerto Rico at Maguey, with financial support from the U.S. Office of Naval Research. The conference was attended by representatives from the Marine Biological Station and the Government Aquarium at Bermuda, the Lerner Marine Laboratory at

Bimini, the Bellairs Institute of McGill University in Barbados, the Caraibisch Marien-Biologisch Institut in Curaçao, the University College of the West Indies in Jamaica and the Universities of Oriente, Villaneuva and Nacional and the Oficina Hidrográfica in Cuba. Several of these institutes are less than three years old, and this was the first occasion on which most of the participants had met each other. The conference was restricted to laboratories established on islands in the area of reef-building corals of the tropical western Atlantic, which extends from the eastern islands of the Caribbean to Bermuda. Throughout this great sea area the marine flora and fauna and the hydrographic conditions are remarkably uniform, and in consequence the participants found a considerable field of common interest in the problems with which they are concerned.

The conference was felt to be of such value by the participants that a permanent Association of Island Marine Laboratories was established to keep its members in touch with each other, to exchange information and to co-operate in research programmes that affect the whole area. The Association hopes also to arrange visits and exchange of experts in special fields of study. A publication is to be issued shortly, giving information on the facilities for research offered by the member institutes, and consideration is being given to the founding of a journal to publish papers on the biology of the area. It is hoped to hold the next conference of the Association in Bermuda in 1958. Prof. D. M. Steven, of the University College of the West Indies, was elected its first president, and Dr. Juan Rivero, director of the Marine Laboratory of the University of Puerto Rico, its secretary.

English Translations of Russian Medical and Biological Journals

The Pergamon Institute, London (4–5 Fitzroy Square, W.1) and New York, is a non-profit-making foundation which has been formed for the purpose of making available to English-speaking scientists, medical men and engineers, the results of research in the U.S.S.R. It has the support of an honorary advisory council containing the names of distinguished research workers from many different countries. The formation of the Institute was stimulated by the recent report of the United Kingdom Government Advisory Council on Scientific Policy (*Nature*, 178, 1255; 1956), which suggested that only about 2 per cent of British working scientists can read Russian compared with 80–90 per cent who read French and 60 per cent who read German. As part of its activities, the Institute has just started the complete translation of a number of Russian medical and biological journals, the first to appear being the *Journal of Microbiology, Epidemiology* and *Immunobiology* (from Vol. 28, issued in monthly parts; annual subscription £18), *Biophysics* (from Vol. 2, eight issues per annum, £10 14*s*.), *Problems of Virology* (Vol. 2, bimonthly, £7 p.a.) and *Problems of Hematology and Blood Transfusion* (Vol. 2, monthly, £7 p.a.). The *Physiological Journal of the U.S.S.R.* (Sechenov) and *Problems of Oncology* will also be translated.

Publication is supported financially by the National Institutes of Health, United States Department of Health, Education and Welfare. It has been possible to arrange for early copies of the journals and the original illustrations to be made available from the U.S.S.R. with the result that publication has been astonishingly prompt: some of the original papers bear dates in late 1956. The translations read easily and the printing and production are of high quality. The bibliographies of the papers are set out very clearly and they show that Russian workers are making a lot of use of journals from English-speaking countries. These translations will help to make the transmission of information a more mutual one. The continued success of the scheme will depend on a sufficient supply of translators to carry on the work, and the Institute invites offers from suitably qualified people who would be willing to help. Translations of individual papers in Russian, and also of some scientific journals, are already available from Consultants Bureau, Inc., 227 West 17th Street, New York, but the Pergamon Institute has the advantage that it is a non-profit-making body.

Biochemistry of Sulphur

A colloquium on the biochemistry of sulphur was held in May 1956 in the biological station at Roscoff, at which thirty-four invited specialists from seven countries took part. The papers which were given and the discussions which followed have now been published (Colloque sur la Biochimie du Soufre, Roscoff, 14–18 Mai 1956. Pp. 244. Paris: Editions du Centre National de la Recherche Scientifique 1956). In his introduction, Prof. C. Fromageot points out that in living matter sulphur occurs in a variety of forms—as thiols, disulphides, thioethers and heterocyclic compounds, whereas phosphorus occurs only as orthophosphates or their anhydrides. New techniques, particularly those of chromatography and the use of radioactive sulphur, have led in the past few years to a rapid increase in knowledge of the biochemical transformations and catalytic actions of substances containing sulphur. The main emphasis at the meeting was on sulphur compounds in the higher animals, though the first two sessions contained papers on the sulphur metabolism of micro-organisms. In the third session Prof. P. Handler dealt with the last stages in the oxidation of cysteine, and described experiments leading to the unexpected discovery that hypoxanthine may act in conjunction with lipoic acid as a reducible carrier in the oxidation of sulphite in animal tissues. Sulphatases were discussed by K. S. Dodgson and by B. Spencer and the metabolism of steroid sulphates by A. B. Roy. Administration of inorganic sulphate containing radioactive sulphur to some of the higher animals showed that the sulphur is incorporated into organic compounds in a manner which differs according to the species of animals used (P. B. Pearson). Ruminants incorporated inorganic sulphur at a rate sufficient to be of nutritional importance, probably owing primarily to activities of micro-organisms in the alimentary tract. This paper produced a lively discussion which included the plea that the term 'detoxication', with its teleological implications, should be dropped from the biochemical vocabulary. Among other topics included in the Colloquium were the metabolism of sulphur in relation to vitamins of the B group, the metabolism of cystamine, and enzymic reactions involving thioesters. In the review copy of this book, pages 113–128 were omitted and duplicates of other pages bound in their place.

Indonesian Museums

Mr. J. Irwin has recently summarized the conclusions drawn during a six-month Unesco mission to Indonesia in an article in the July issue of the *Museums Journal*. After a rapid survey of the present position, he was convinced that the most urgent problem was the lack of trained personnel. It was thus recommended that potential candidates should first receive a basic training course in Indonesia—probably a year at the Djakarta Museum—and then be awarded a travelling fellowship in Europe or America to specialize in some particular aspect relevant to Indonesia's specific needs. Mr. Irwin further considered that if museum development is to receive substantial support from the authorities it must play a positive part in the major national aim of fostering a consciousness of the Indonesian motto, "Unity in Diversity". To this end about thirty acres of land near the capital has been purchased for a national museum, and around the periphery it is planned to erect spectacular examples of traditional domestic architecture brought from the main cultural regions of Indonesia. Each of these houses is to be set in its own botanical setting and furnished in its own traditional way. This part of the scheme illustrates the diversity of Indonesian culture, while the unity would be shown in a modern functional building sited in the centre of the park, illustrating the links in Indonesia's cultural development. The whole project has been designed to be completed in ten years, and the acquisition of the land for 1,250,000 rupees is an indication that the authorities are keen to accept the recommendations of the Unesco report.

Mathematics in Industry

The alarming shortage of competent mathematical teachers in British schools is already responsible for a shortage of good young mathematicians available for industry. As the language of science, mathematics is a major pillar of any technological economy, and the Mathematical Conference for Schoolteachers and Industrialists held at Trinity College, Oxford, during April 8–18 was based on this brute fact. The "Abbreviated Proceedings" (pp. 112. London: *Technology*, Times Publishing Co., Ltd., 1957. 2*s*. 6*d*.) therefore contains matter of national interest and importance, meriting the attention of parents, administrators and politicians. Industrialists expounded their mathematical needs and the wide variety of mathematical applications; research scientists helped to bridge the gap between pure mathematics and technology; teachers considered how content and exposition of a school curriculum might be modified to provide an adequate preparation for industrial mathematicians without thereby losing sight of the concept of mathematics as a part of general culture and education. Teachers present at the conference spoke warmly of the great stimulus and encouragement received, and hoped that further meetings may be arranged.

Nature Conservancy Awards for 1957

The Nature Conservancy announces the following awards of research studentships for postgraduate training in ecology, tenable for periods up to three years at the universities shown: *Botany*, Miss B. C. Barrett (Durham–Newcastle), Miss D. C. Munroe (London), Miss C. M. D. MacDonald (Oxford), E. I. Newman (Cambridge), G. A. M. Scott (University College of North Wales, Bangor); *Zoology*, J. M. Cherrett (Durham), L. M. Cook (Oxford), I. E. Efford (Oxford), J. E. C. Flux (Aberdeen), J. F. Peake (London).

Announcements

Dr. Nicholas Kurti, of the Clarendon Laboratory, Oxford, is the first recipient of the Fritz London Award, which will be given biennially for distinguished research in low-temperature physics and chemistry as a memorial to the late Fritz London, of Duke University, North Carolina. Arthur D. Little, Inc., industrial research consultants of Cambridge, Mass., is sponsoring the tribute this year and will sponsor the award again in 1959.

The next meeting of the American Association for the Advancement of Science will be held during December 26–30 in Indianapolis. Further information can be obtained from the A.A.A.S., 1515 Massachusetts Avenue, N.W., Washington 5, D.C.

The Plastics and Polymer Group of the Society of Chemical Industry is to hold a symposium in London during April 15–17, 1958, on "The Physical Properties of Polymers". The symposium will be devoted to recent advances in the fundamental investigation of mechanical and electrical processes in polymers, and to the physical testing of plastics, rubbers and fibres, with particular reference to dynamic and relaxation methods. All inquiries and offers of contributions to the symposium should be made to the honorary recorder, Mr. K. H. C. Bessant, c/o The Distillers Co., Ltd., Great Burgh, Epsom, Surrey.

A course of lectures on "Observations of Dislocations in Metals and Inorganic Crystals" will be given in the H. H. Wills Physical Laboratory, University of Bristol, during March 24–29, 1958. The lecturers will be Prof. F. C. Frank, Dr. J. W. Mitchell, Dr. J. F. Nye, Dr. N. Thompson and other research workers. Those attending can be accommodated in Wills Hall, one of the halls of residence of the University. Further information can be obtained from the Director, Department of Adult Education, University of Bristol.

An International Conference on High Polymers will be held at the University of Nottingham during July 21–24, 1958. Proceedings will be divided between two sections meeting simultaneously, the main subjects to be discussed being: *A*, reaction mechanisms and kinetics, heterogeneous polymerization (including trapped or inactive radicals), production of graft and block co-polymers; *B*, physical, thermodynamic and mechanical properties. Those wishing to present a paper should submit an abstract of 200–300 words in English, French or German, not later than December 15, 1957, to the International High Polymer Conference, The University, Manchester 13. The organizing committee will make up its programme by selection from these abstracts, and will invite the authors to prepare their completed papers not later than March 1, 1958. The full proceedings of the Conference, including the discussion, will be published in the *Journal of Polymer Science*.

MATERIALS FOR NUCLEAR ENGINEERING

THE summer meeting of the Physical Society took the form of a two-day conference on "Materials for Nuclear Engineering". It was organized with the help of Associated Electrical Industries Ltd., and was held at the Associated Electrical Industries Research Laboratory, Aldermaston, during July 11–12.

Dr. T. E. Allibone (director of the Research Laboratory, Associated Electrical Industries Ltd.) opened the conference with a general review of the problems encountered in finding suitable materials for nuclear engineering and their importance in the future development of nuclear energy. He considered it very appropriate for the Physical Society to arrange a conference to discuss the physical properties of materials rather than their technology, for the technological questions would no doubt be answered when the underlying physics was better understood.

The papers delivered at the Conference could be classified into four groups: irradiation effects, physical properties of particular metals, liquid metals and the structure of materials.

Irradiation effects were discussed in four of the papers. Dr. A. H. Cottrell (Atomic Energy Research Establishment, Harwell) considered the effect of irradiation on creep. The importance of this problem was due to the fact that materials, although not subject to movement, had to withstand stresses at fairly high temperatures. The main effects of irradiation were the production of vacancies and interstitials, structure changes, radiation hardening, and the generation of internal stresses. Since the movement of vacancies was the vital factor affecting the creep-rate, it might be expected that if the coefficient of self-diffusion was high, poor creep properties would result, and he suggested that it was a good idea to use materials for which the diffusion coefficient is less than 10^{-15} cm.^{2}/sec. Cottrell calculated that the effect of irradiation on the diffusion coefficient kept its value less than the critical 10^{-15}, and thus there was no significant change in creep properties due to this cause, as was in fact found in the early experiments. Structure changes could be caused by irradiation; for example in the uranium/molybdenum system, irradiation changes the structure from α-uranium + ε to γ-uranium in the quenched alloy at 400° C. Radiation hardening helped creep properties, but usually this occurs at temperatures lower than those where creep is important. Internal stresses can cause creep when they arise in a non-Newtonian body, and may be caused by thermal gradients, growth strains, thermal cycling strains and swelling. Growth strains are caused by a non-cubic crystal changing its shape, and in α-uranium this change is an expansion in two directions plus contraction in a third; the increased creep-rate has been observed experimentally.

Irradiation effects in insulating crystals were considered by Prof. R. W. Ditchburn (University of Reading). In order to understand the effects of pile neutrons it is desirable to study the displacement of single atoms to form vacancy interstitial pairs. This can also be done by using electrons of controlled energy (about 0·5 MeV.), but local heating must be reduced in order to avoid partial annealing. The damage produced is studied by measuring the change of some physical property, and optical absorption is a particularly powerful method of investigation for it gives the energy-levels and the relative numbers of displaced atoms. For diamond, it is possible to investigate the displacement of single atoms. Experiments showed that heating at 450° C. causes some annealing; at 600° C. it causes the formation of pairs of defects and at 900° C., after prolonged heating, agglomerations of defects occur. Another valuable technique for the understanding of irradiation damage is the study of the polarization of irradiation-induced fluorescence, and by this means it is possible to deduce the orientations of defect systems.

Dr. G. B. Greenough (U.K. Atomic Energy Authority, Windscale) discussed irradiation effects in graphite. When it is exposed to neutrons with energies greater than 100 eV. at temperatures below 150° C., vacancies and interstitials are created, which cause changes in the physical properties. Thus the thermal and electrical conductivities decrease by factors of thirty and three respectively, and Young's modulus and the breaking strength increase by a factor of two. Changes in the external dimensions of a block of graphite also occur due to the fact that the c and a interatomic spacings increase and decrease respectively. Annealing the graphite restores its physical properties, but temperatures in excess of 1500° C. are required for full restoration. The annealing process, which causes the return of interstitials to vacant lattice sites, is accompanied by a high energy release. Dr. Greenough stressed the fact that experimental work is somewhat difficult because the properties of unirradiated graphite depend on its method of manufacture, and thus many samples must be investigated.

Dr. R. S. Barnes (Atomic Energy Research Establishment, Harwell) and Dr. A. T. Churchman (Associated Electrical Industries Ltd.) considered the structural effects in solids of nuclear-induced gases. These, which are generally of the rare-gas type, result from nuclear reactions caused by neutron irradiation, and can cause drastic structural changes in the material which is used. For example, boron and boron carbide, both of which are brittle, fragment upon irradiation with thermal neutrons, there being a helium-producing reaction in boron. It is believed that in each of these materials, the included gas diffuses to flaws, so creating a gas pressure which causes crack propagation resulting in fragmentation. The behaviour of deformable materials containing gas is quite different from that of brittle materials. From experiments on irradiation of beryllium by 40 MeV. α-particles producing helium, and of uranium by thermal neutrons, when the rare gases xenon and krypton are among the gaseous products, it was concluded that at low temperatures large quantities of the inert gases can exist in materials without causing an appreciable increase in volume. Heating permits the precipitation of the gas into bubbles if the material is ductile, or it cracks if it is brittle. The size and number of bubbles or cracks are determined by the presence of nuclei, the diffusion coefficient and the solubility of the gas. It was found that

the increase in volume caused by bubble formation depended upon the distribution of the gas, and that if the bubbles were small and numerous the gas was contained at a very high pressure and the resulting increase in volume was small. But if the same amount of gas was distributed in a few large bubbles, a large volume increase resulted. Such increases in volume could be troublesome in a reactor since, for example, it may cause splitting of the fuel can, allowing corrosion, or impeding the flow of coolant.

The physical properties of particular metals were dealt with in two papers. Mr. G. C. Ellis (Atomic Weapons Research Establishment, Aldermaston) delivered a paper on beryllium and in particular discussed the problem of its brittleness. This metal has attractive nuclear and high-temperature properties, but the main difficulty in its use is its relatively low ductility. Mr. Ellis reviewed the metallurgy of beryllium and considered the methods of producing the ductile metal by controlling the grain orientation. He considered that a considerable improvement might be obtained by removing interstitial solute impurities. However, the problem of purification, especially on a large scale, was likely to prove very difficult, but he suggested that there was sufficient justification to make the necessary effort.

The physical properties and alloying behaviour of plutonium were discussed by Dr. M. B. Waldron (Atomic Energy Research Establishment, Harwell). He stressed that the two most distinctive features of plutonium were its radioactive toxicity, which with its inherent difficulties slowed up the progress of research with the metal, and its allotropic behaviour, for six different forms of the metal have been identified. The relative stabilities of the various forms are of particular interest. Although certain similarities existed between plutonium and uranium phase diagrams, to assume direct analogies between the two was very dangerous. Dr. Waldron produced data on the specific heat at low temperatures and on dilatometric studies. Of particular interest was the negative coefficient of thermal expansion of the face-centred cubic δ-phase. This is unique for a metal with an isotropic crystal structure and emphasized the abnormal behaviour of the plutonium atom. In the discussion on the paper, Dr. E. Dempsey (Atomic Weapons Research Establishment, Aldermaston) contributed data on the Young's modulus and oxidation properties of the metal.

During the course of the conference it was evident that the subject of liquid metals was very important, and three papers were read on various aspects of it. Dr. B. R. T. Frost (Atomic Energy Research Establishment, Harwell) delivered a paper on the wetting of solids by liquid metals. He outlined the role taken by liquid metals in reactor technology, and stressed that in all the fields of use, wetting problems arise. This usually means that it is necessary to know the condition at the interface between the liquid metal and the solid metal under consideration. Definitions of wetting are usually given in terms of the contact angle θ, and for metallurgical application it is convenient to use $\theta = 90°$ as the dividing line between 'wetting' and 'non-wetting'. It depends on the balance of solid and liquid surface tensions, or in other words on the degree to which the solid and liquid interact. For a quantitative understanding of the process of wetting it is necessary to measure the surface tensions of the solid and liquid, and also their interfacial tension, and Dr. Frost gave an account of the various methods of measuring these quantities. He discussed the inducement of wetting by the addition of elements (wetting agents) to the liquid metal, and mentioned the part that fluxes can play. As particular examples of the problems encountered, he mentioned the wetting of particles in slurries where the liquid metal acts as a fuel carrier, the wetting of surfaces in an electromagnetic pump, wetting during brazing, and also the problem of corrosion.

A special type of corrosion is mass transfer, which was discussed in a paper by Dr. A. K. Covington, Dr. G. A. Geach and Dr. A. A. Woolf (Associated Electrical Industries Ltd.). It is a process whereby a liquid or vapour takes up a solid at one point by chemical reaction or solution, transfers it by mass flow to a point where conditions differ and there allows it to be re-deposited. This deposition may be a consequence of change in temperature or it may be due to chemical reaction with another material. Mass transfer can be one of two types. Thermal gradient mass transfer arises when a liquid metal is confined in a solid metal system subjected to a temperature gradient. The difference in solubility of the container material in the liquid metal at different temperatures results in solution in the hotter portions and precipitation in the colder portions, with subsequent rapid blockage of the system. Ways of reducing such transfer were discussed with particular reference to the formation of inhibiting layers of zirconium nitride on steels. Isothermal mass transfer can occur when two dissimilar metals are placed in a liquid metal. Some affinity between a metal soluble in the liquid metal and one less soluble can lead to the formation of a layer of an intermetallic compound or solid solution on the less soluble metal. For example, aluminium has been observed to transfer on to the surface of pure iron, causing the formation of Fe_2Al_5. As in the inhibition of thermal mass transfer by zirconium nitride, the presence of free nitrogen in the iron may prevent transfer of aluminium probably by the formation of an aluminium nitride film.

The third paper connected with liquid metals was that by Mr. J. S. Hilditch (British Thomson-Houston, Rugby) on the electromagnetic pumping of liquid metals. The great advantage of the electromagnetic pump over its mechanical counterpart is that it allows liquid metals to be circulated at high temperatures without glands, seals or moving parts. Principles upon which such pumps operated were described, and the performance figures given for some d.c. and a.c. conduction pumps. Alternating current induction pumps of the spiral, flat, annular and re-entrant types were also explained. The future of electromagnetic pumps appeared to be in their large-scale application to power reactors, and for bismuth the choice lay between the d.c. conduction pump with homopolar generator and the low-frequency a.c. conduction pump with separate transformer, or possibly the rotating-magnet induction pump. However, for sodium and sodium potassium alloys, where the resistivity was lower, the flat or re-entrant annular type of linear induction pump was favoured.

The structure of materials was dealt with in two papers. Dr. J. Friedel (Ecoles des Mines, Paris) discussed possible causes for allotropic transformations in the heavier elements. He first compared the behaviour of these elements with those of the first

period transitional metals. In the period commencing with francium and extending to americium, a big drop in the melting point occurred at uranium, while very complicated crystal structures existed for those elements showing allotropy. This was due to the presence of homopolar unsymmetrical binding caused by the filling of the *f*-shells as well as the *d*- and *s*-shells. In uranium, this operation of both *d*- and *f*-states gave rise to an unsymmetrical crystal structure, while on heating the *d*-bonds were broken, causing anomalies in the linear expansion, abnormally small electrical resistivity and, at higher temperatures, a change in crystal structure to β- and γ-uranium. Americium existed in only one form with a peculiar double hexagonal close-packed crystal structure, indicating the beginning of a rare-earth behaviour with the electrons not forming bonds but existing in independent shells. Thus Dr. Friedel considered that the elements of low atomic number in the range had a structure similar to the transition metals, with a mixture of *d*- and *f*-orbitals giving rise to directional homopolar bonding, while for plutonium and americium the structures were more analagous to the rare earths, with the electrons in the *f*-shells predominating over those in the *d*-shells.

Some aspects of structure and solid solution were considered by Dr. G. K. Williamson (Atomic Energy Research Establishment, Harwell). The alloying behaviour of uranium was discussed with the view of determining its behaviour empirically. There was negligible solubility in the complex α- and β-structures and in the body-centred cubic γ-structure; it was restricted to the transition elements (Group IV*A* onwards). The Hume-Rothery size factor rule was obeyed for the elements of the first two long periods, but this is not so for elements of the third long period, and the reason for this is not yet understood. Uranium forms a large number of compounds, but it seems remarkable that, apart from those with a special size factor, no compounds are formed with elements of lower electronegativity. The compounds formed cannot be accounted for on the basis of electron compounds, for the effective valency of uranium is uncertain.

The conference was concluded with a talk by Sir John Cockcroft (Atomic Energy Research Establishment, Harwell) on the place of metallurgical development in the atomic programme. Future reactors would require fuel elements operating at higher temperatures, with higher heat ratings and burn-ups than those at present in use. Ratings could be increased in two ways. The surface area to volume ratio of the fuel element could be increased by using clusters of thin rods or plate fuel elements, but these were limited by fabrication problems or by the increase in the ratio of canning material to fuel. Again, the fuel element temperature could be increased, but this causes swelling in metallic uranium at 550–600° C. This difficulty could be overcome by using a 14 per cent molybdenum alloy, but such a degree of alloying is too high for thermal reactors, although it may be applicable for use in fast reactors. At temperatures above 600° C., it is necessary to use ceramic materials, uranium oxide or uranium carbide, as fuel, but these have their difficulties. With such high operating temperatures, beryllium cans are required and experiments have been carried out to test their ability to withstand such temperatures. By a combination of methods, it is hoped to increase the rating to 5–10 MW. per ton compared with 1·3 MW. per ton for the Calder Hall reactor. Sir John mentioned the more advanced reactor types—the Dounreay fast reactor, the high-temperature gas-cooled reactor and the liquid fuel reactor—and concluded with a consideration of other problems, including those of irradiation damage of the steel pressure vessels, and stress corrosion difficulties.

T. E. Allibone

THE CHEMISTRY OF TERPENOID COMPOUNDS

A SYMPOSIUM on recent advances in the chemistry of terpenoid compounds, organized by the Chemical Society, was held in the Chemistry Department, University of Glasgow, during July 11–12. A marked feature of this symposium was the emphasis placed on ideas and methods which had scarcely entered the field even as late as 1950. Terpene chemistry was the first chemistry of natural products in which a wide range of related substances was examined, and the emphasis for many years was on structures. The first great generalization was the 'isoprene rule', which has recently been expanded into the 'biogenetic isoprene rule', and nowadays it is evident from the conference that chemists are not content merely to determine structures but wish to correlate these structures with those of the isoprenoid compounds in general. The term 'biogenesis' was mentioned in some connexion in almost every paper. Another feature was the universal dependence on physical methods for suggesting probable structures: infra-red and ultra-violet spectra and optical rotatory determinations are some of the chief tools used. Attempts at total synthesis of terpenoid compounds were described in only two papers.

The attendance at the symposium was international, with representatives from several countries including Australia, Canada, France, Great Britain, New Zealand, Switzerland and the United States.

The first paper, on the structure of the sesquiterpene ketone zierone (Prof. A. J. Birch, D. J. Collins, R. P. Hildebrand, A. R. Penfold and M. D. Sutherland), is an example of the use of newer methods based largely on absorption spectra. The substance is a derivative of a new natural azulene, and the chief method of structure determination was by dehydrogenation, by methylation at the carbonyl group and adjacent to it, and dehydrogenation, and then a study of the spectra of the resulting azulenes. Some probable conclusions can even be reached as to the stereochemistry of the whole molecule by a study of the spectra of dihydrozierone diol produced by osmic acid oxidation. Zierone is chiefly notable in that although it is isoprenoid one of the units is reversed from the normal head-to-tail linkage. In this connexion also Prof. F. Sörm discussed the structure of acorone and laserpitine, both rather remarkable ketonic sesquiterpenes. The latter is an isocadalene derivative with one reversed unit, the former a

spiro-diketone with a regular linkage, which by migration reactions of known type could be rearranged to an isocadalene derivative. It is suggested that it might in fact represent the type of linkage between the two series. The use of spectra was again well illustrated in a number of connexions, but notably in the observation that the infra-red spectrum of the fully reduced acorone showed the compound not to belong to any known series of bicyclic sesquiterpenes.

A paper by Prof. W. Cocker illustrates another aspect of the effect of light: its ability to isomerize some substances to interesting structures of higher energy-level. In this case the product obtained by irradiation of santonin in aqueous solution and its transformation products were discussed, and it was concluded that the initial product contains a cyclopropane ring. Among other use of spectra, the infra-red adsorption was used for counting the number of methyl groups present. It is an interesting speculation as to how far similar reactions in plants possibly catalysed by other substances may explain the production of 'improbable' compounds of this nature. Work on irradiation of santonin is proceeding in at least three other laboratories, and the symposium has no doubt facilitated the correlation of these researches. Other irradiation work was later reported by Dr. G. Büchi.

In a not too distantly related field, Dr. F. J. McQuillin considered the autoxidation of sesquiterpenes of the eudesmane series containing an $\alpha\beta$-unsaturated ketone system; similar products were obtained by the use of alkaline persulphate. With carboxyl groups in the correct position lactone rings can be produced in the same position as in santonin. In this case the autoxidation was much influenced by the configuration of the side-chain.

A number of problems of triterpene structure were presented. Prof. E. R. H. Jones (with T. G. Halsall, W. J. Dunstan and H. Fazakerley) discussed the chemistry of hydroxyhopanone and concluded that it is pentacyclic, but they could not relate the nucleus to a known series. It is thought possibly to be related to zeorin. All the standard methods, including optical rotatory dispersion, were used in this work. Prof. G. Ourisson (with P. Crabbe, T. Takahashi and M. Palmade) used classical-type methods to elucidate the structure of dipterocarpol. This was converted to a steroid-hormone analogue (8β: 14α-dimethyl-18 nor-nortestosterone) which is biologically completely inactive. It was suggested that this is due to distortion by axial 8 : 18-methyl groups. The structure of katonic acid (3α-hydroxyolean-12-en-29-oic acid) was elucidated by Dr. F. E. King (with J. W. W. Morgan) by relating it to the β-amyrin series. The authors appeared not to know whether to be pleased with a comparatively easy solution of the problem or disappointed that something more fundamentally novel was not involved.

Prof. H. Erdtman (Stockholm), who was presented with the Flintoff Medal for his achievements in the field of chemical botany, considered very carefully possible uses of chemical constituents in taxonomy. The chief note sounded was of hope, tempered with caution, but obviously a great deal of work must be done before firm conclusions can be reached.

A totally different aspect of the subject was treated by Prof. E. Wenkert (with B. G. Jackson), who discussed the problems encountered in synthetic work in the tricyclic diterpene field. Although the original aim was not completely achieved, Prof. Wenkert in characteristic fashion turned to advantage apparently incidental observations to provide a reductive (lithium–ammonia) method for the hydrolysis of hindered esters, and in a brilliant exposition extended his biogenetic theories in postulating valid analogies between Friedel–Crafts reactions and biogenetic processes.

Another synthetic problem was discussed by Prof. R. A. Raphael in considering a most ingenious example of the indirect approach in work designed to lead up to the total synthesis of iridodial and related substances.

Natural diterpenes were not neglected. T. G. Halsall (with J. D. Cocker) described the structure and stereochemistry of labdanolic acid and discussed the possible biogenetic significance of the failure of diterpenes and triterpenes to occur together. Dr. W. B. Whalley (with Miss A. Harris, and Prof. A. Robertson) described work leading to the structure of rosono—and rosenono—lactones, the first fungal diterpenes to be structurally elucidated.

The structure of the complex bitter principle limonin, $C_{26}H_{30}O_8$, was discussed by Dr. O. Jeger, who was not sure whether it is a triterpene less four carbon atoms or a diterpene plus six carbon atoms. It contains a furan ring, two carbon rings, two lactones and probably two other ether (epoxide ?) rings. Several of its complicated reactions were formulated in a reasonable manner, and a race is now anticipated between organic chemists and X-ray crystallographers for a final solution.

Some extremely energetic three-dimensional musical chairs was involved in three papers dealing with very complex ring-structures. On the basis of this work there is certainly no need for natural-product chemists to be apologetic about their ability to deal with reaction mechanisms. Prof. A. Büchi confirmed the structure of 'carvone camphor' obtained by illumination of carvone, and showed that it contains a cyclobutane ring the presence of which leads to most interesting rearrangements. Prof. K. Wiesner (alkaloids from *Aconitum napellus*) and Prof. R. C. Cookson (with M. E. Trevett) (delpheline) showed that terpene chemists will in future have to take into account the chemistry of nitrogen. The beautiful and complex three-dimensional networks discussed require a three-dimensional mind to cope with them. The reporter was feeling two-dimensional at the time.

The use of infra-red spectra in all the preceding work was appropriately emphasized by Dr. R. N. Jones, who discussed a new method for use with micro-quantities. He also described some potential uses of the method of 'subtracting' spectra.

From the scientific point of view, including selection of subjects, methods of presentation and general smooth running, the symposium was the most successful the author has attended. The high attendance to the last word, and generally expressed opinion, confirm this impression. Discussion was lively and interesting, with particularly notable contributions from the United States. Several very enjoyable social occasions were also provided: a dinner in the beautiful loch and hill country of Aberfoyle, and a Scottish evening with Scots songs and dances and other characteristic Scotch products was attended by kind invitation of Imperial Chemical Industries (Nobel Division). The organizing committee is to be heartily congratulated, and thanks are particularly due to Dr. A. Payne, the local representative of the Chemical Society.

A. J. Birch

SCIENCE TEACHING AT KING'S COLLEGE, TAUNTON

ON July 6, H.R.H. Princess Margaret opened a new science block, consisting of four laboratories, two preparation rooms and a science library, at King's College, Taunton. Incorporated in the front of the new building is a memorial gateway to those Old Boys of the College who gave their lives in the Second World War.

There is in the nature of the memorial a special significance for those connected with the College, for King's was probably the first school in Britain in which science was seriously taught. The person responsible for this early start was the Rev. William Tuckwell, headmaster of Taunton College School, as the school was then called, during 1864–77. When Tuckwell was first appointed head, the school was situated in the old buildings erected in 1522 by Bishop Fox. They are now Taunton's municipal buildings. Within a year of his acceptance of the post, Tuckwell had bought the land where the school now stands, south of Taunton. The site was acquired in 1865 and the new buildings opened in 1870. Tuckwell had introduced the teaching of physical science in 1865 and he built his laboratory in 1869.

An eminent botanist and a scholar of English literature, Tuckwell wrote books on both these subjects, and, during his term of office as headmaster, delivered a course of public lectures and read papers on the teaching of science before the British Association. His revolutionary ideas met with much reactionary opposition among the classically prejudiced. In his paper "Science-teaching in Schools", read before the British Association at Exeter in 1869 (which was published in the first issue of *Nature*, November 4, 1869) he stated, referring to his science pupils, ". . . they will pass out into the world immeasurably superior to their contemporaries who know not science, with doors of knowledge opened which can never again be closed; with a fund of resource established which can never be exhausted; with minds in which are cultivated, as nothing else can cultivate them, the priceless habits of observation, of reasoning on external phenomena, of classification, arrangement, method, judgment".

Tuckwell held that the syllabus should include two years of mechanics, two years of chemistry and one year of botany, and that any further time at school should be spent studying physiology. He recommended that three hours of science should be taught each day. For the teaching of mechanics, he maintained that the necessary apparatus should include ". . . a good air-pump, a set of pulleys, models of the force-pump and the common pump"; using as a text-book Newth's "Natural Philosophy". He took his pupils on visits to various 'manufactories', because he believed that they ought to have practical knowledge of the application of this apparatus in everyday life.

For chemistry he considered a laboratory essential: ". . . No matter how rough or shabby a room, so that it will be well ventilated, have gas and water laid on, and will hold from sixteen to twenty boys . . .". His benches cost about £4 each. ". . . The general laboratory stock, including a still, a stove or furnace, gas jars, a pneumatic trough, a proper stock of retorts, crucibles, tubing, etc., and the necessary chemicals will cost under £12. Each pair of pupils must have also between them a set of test tubes, a washbottle, a spirit lamp . . . and twenty-four bottles of test solutions, while each boy has his own blowpipe, tripod and stand, pestle and mortar, and three beakers. These will cost each boy about eight shillings. . . . The text-book used should be Roscoe's, or Williamson's, and a large black board is quite indispensable . . .".

He suggested such books as Lindley's "Descriptive Botany" as a text-book for botany; and for botanical experiments, he thought that ". . . every boy should be furnished with a small deal board, a lens, and a sharp knife . . . Flower trays . . . should be kept constantly in use . . . Their cost per tray, holding eighteen bottles, is under two shillings . . .". He also deemed it necessary to possess a skeleton, the cost of which he estimated at £5.

Tuckwell also thought that schools studying science should have a set of meteorological instruments, which he estimated would cost between £16 and £20. Also he suggested that a special room should be devoted to the collection of scientific specimens, such as minerals, fossils, zoological specimens and physiological preparations; the driest corner of the room he assigned to the herbarium. He recommended a small library and a botanical garden.

He probably put some of these ideas into practice even when

(*Continued on p.* 489)

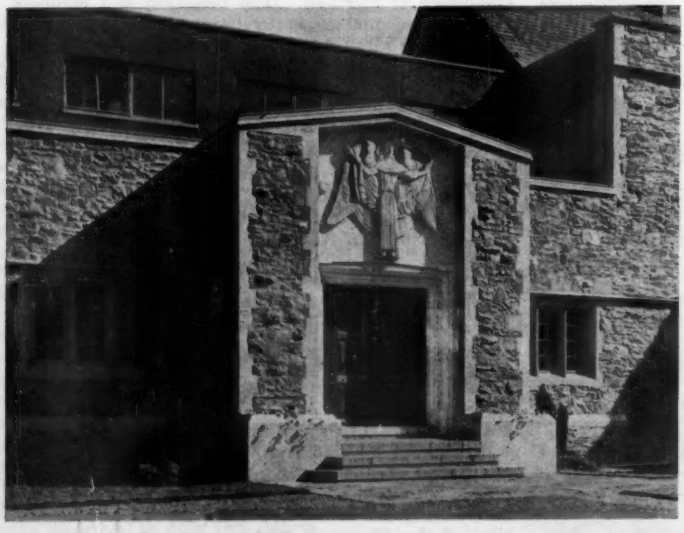

Fig. 1. Memorial Gateway and entrance to new science block at King's College, Taunton

BRITISH ASSOCIATION MEETING IN DUBLIN

TECHNOLOGY AND WORLD ADVANCEMENT*

By Prof. P. M. S. BLACKETT, F.R.S.
President of the British Association

I

SCIENTISTS have often been reproached for not concerning themselves sufficiently with the effects —for good or ill—of their discoveries on society. This charge can scarcely be laid against recent presidents of the British Association. Most of my illustrious predecessors of the past decade have dealt in one way or another with the impact on contemporary society of the vast eruption of scientific and technological discovery, which is by far the most historically significant facet of our epoch.

Last year a special committee was appointed by the Council of the Association to re-formulate the Association's objectives in the light of present-day conditions—so different from those of 1831, the year of our founding. One of its recommendations was that still more stress should be laid on the social consequences of modern science and technology. I think that this advice is wise. However, I cannot deny that to follow it is to risk becoming controversial. I do not mean controversial over matters of scientific fact and theory, but controversial in a plain political sense.

For in science and technology lies the key to wealth; and the creation and distribution of wealth are the warp and woof of the many-patterned texture of politics. Many scientists and engineers—let us be quite frank about it—have in the past deliberately avoided thinking more than they can help about the social consequences of their collective achievements: just because, to do so, would sweep them into the troubled waters of political controversy. Some of our scientific predecessors were far less inhibited. It is worth recalling the forthright dictum in 1802 of the young Humphry Davy: "The unequal division of property and labour, the differences of rank and condition among mankind, are the sources of power in civilized life, its moving causes and even its very soul". Davy, who has been called the major prophet of the new class of applied scientist, had no scruples about expressing frankly his views on a major problem of his day. I will try to emulate his forthrightness in expressing my views on what seems to me to be the major and very different problem of ours.

It has often been held that the development of natural science and technology gives rise to political controversy only because of the backwardness of the social sciences. If the social consequences of technology, and indeed the problems of society in general, could be studied with the same objective impartiality which has worked such miracles in the physical world, then, so it is alleged, much of the sordid controversy of politics could be avoided.

This is less than half a truth. While all will echo Lord Adrian's plea at Oxford in 1954 for a more rapid development of the social sciences, one must be clear about their limitations. The facts discovered by social scientists—including economists—and the theories which they elaborate, cannot by themselves define the objectives of social policy: what the goals should be must ultimately rest on a judgment of values by individuals. Such judgments of priorities, when made by social groups, become political programmes.

When technological advance increases a nation's power to produce wealth, how should this be used? How much should be devoted to more material goods and how much to more personal leisure and to gracious living; how much to providing greater consumption now and how much should be set aside for the future; how much to greater opportunity for youth and how much to greater security or luxury for old age? How and to what extent should different kinds of ability and enterprise be rewarded? The social scientist can assist us to think rationally about such alternative uses for our material wealth and may help us to attain any end we choose; he cannot make the choice for us.

II

Though few to-day in the rich Western world are naïve enough to think that an increase of material wealth necessarily results in a corresponding increase of human happiness and personal fulfilment, yet few would deny the validity of using a nation's material wealth as a measure of its potential welfare.

One of the most striking features of our contemporary world is the very uneven distribution of material wealth. Leaving aside the countries in the Soviet orbit, as needing separate discussion which will not be attempted, we have at one extreme the highly industrialized countries of Europe and of North America and Australasia, with, in round figures, a population of 400 million, and an average income a head of £300 a year (at 1949 prices). For Europe alone the average is £200 a year. Within this group of relatively rich Western countries there is, of course, a wide range of average income, from one-third to more than double the average. Three hundred years ago, the pre-industrial countries of Europe had a real income a head of not much more than one-tenth of what it is to-day. In the past half-century their wealth has been increasing at a rough average of nearly 2 per cent a head every year, thus doubling in rather less than forty years. This huge and historically rapid rise in individual wealth has been accompanied by a very large rise in population.

* Presidential address delivered in Dublin on September 4 and appearing in the September issue of the *Advancement of Science*.

In marked contrast to the rich West are the still pre-industrial countries, particularly those of Asia, Africa and South America. These—excluding Soviet China—have a population of some 1,000 million people and an average income of about £20 a year a head, that is, one-tenth that of Europe and a still smaller fraction of that of the West as a whole. Moreover, this low income does not on the average seem to have increased much during the past three centuries and may at times have fallen, despite the revolutionary improvements in world technology as a whole during this period. It seems from such figures that 300 years ago the standard of life in at least the most advanced nations of the East, such as China, India or Persia, must have been as high as that of Europe. The gap now of ten to one in favour of Europe is of quite recent origin and is due to the sudden advance of the West, mainly in the past 200 years, whereas the Eastern countries have remained nearly static.

Economists have recently been turning their attention to the complex historical causes which lead a country to transform itself from a static pre-industrial State to a growing industrial one. Making apt use of an aeronautical metaphor, the American economist Rostow has named this vital period of transition 'the take-off into sustained growth'. In Britain, the first country to become fully industrialized, this critical period seems to have been the last twenty years of the eighteenth century.

In a typical pre-industrial country, three-quarters or more of the population may be engaged in agriculture, and wealth tends to remain constant or rises but slowly. Savings and gross investment are low, some 5 per cent or less of the national income, that is, only about enough to maintain a static economy by paying for the depreciation of existing wealth. After 'take-off', savings and gross investment rise until some 15 per cent of the national income is available for gross investment, leaving around 10 per cent for net new productive investment. On the average in the West to-day such new investment results in a rise of total income of about 3 per cent a year. Allowing for the population rise of some 1 per cent, this gives an increase of wealth a head around 2 per cent a year. The fraction of the population engaged in agriculture steadily falls as social development and industrialization proceed, and agriculture itself becomes partly industrialized and so much more efficient. In Britain, the savings and investment required for the 'take-off' were essentially provided by the prosperous classes, who did not spend their surplus wealth on ostentatious living but invested it in productive industry. It is to this fact which Humphry Davy referred in the remark I have quoted.

Everyone recognizes that most scientific and technological innovations can lead to continually increasing wealth only when they become embodied in material things, particularly production goods such as machine tools, chemical and fertilizer plants, transport and communication systems. Nuclear power, the outstanding technical development of this decade, adds to wealth only when nuclear power stations have been built. However, the large amount of capital required is not always appreciated, especially by scientists and inventors, who naturally wish to see their discoveries and inventions used. For heavy basic industries such as steel, oil refining or heavy chemicals, a capital of £5,000–£10,000 a man employed is required; for medium and light manufacturing industries perhaps £1,000–£3,000. Such sums must come from government, corporate or personal savings, that is, at the expense of present consumption; or, alternatively, from external loans. The high capital cost of industrialization is the main reason why the 'take-off' is such a difficult operation for the pre-industrial countries. This is especially so at present because most production goods must be imported, thus making heavy demands on foreign exchange, which, except in a few oil- and mineral-producing countries, is chronically short.

In addition to maintaining its existing wealth, the Western world is saving and investing productively some 10 per cent of its income of £300 a year a head; that is, some £30 a year a head is being invested in additional plant and machinery to create more wealth. The pre-industrial countries of Asia only have about £20 a head to live on, that is, for both consumption and production goods. The West is thus saving more than the East is spending on everything. No wonder that the gap in wealth between the West and Asia is steadily widening. Moreover, most new scientific and technical discoveries or developments tend to widen the gap still more, just because the already rich countries have the capital to make full use of them, but the poor countries have not. For example, under present conditions, the advent of nuclear power must tend to widen the absolute gap still further, because of the high capital expense of nuclear power stations. Decidedly, scientific and technical invention is no fairy wand to wave over a poor country to transform it into a rich one.

III

Anyone who wishes to make himself familiar in detail with the degree of poverty of the underdeveloped countries of the world, particularly those of South-East Asia, India, Pakistan, Ceylon, Burma, Indo-China and Indonesia, will find abundant material at his disposal. In recent years there has been a spate of blue books, white papers, newspaper leaders, speeches, tracts and academic treatises in which can be found statistics, eloquent writing, moral and political fervour. Of special importance were President Truman's 'Point Four' in his inaugural speech in 1949, and the Colombo Plan in 1950.

When after the Second World War these countries threw off alien rule, the ills of South-East Asia—poverty, underfeeding, disease and illiteracy—became important factors in international relations. These new independent countries became free to make their own external and foreign policy, based on their own views of their own interests. They can now choose for themselves with which countries they will trade or with which they will have close diplomatic or military relations. Moreover, no political party in these new Asian countries can hope to maintain itself long in power if it does not succeed in improving the lot of its people. The pre-industrial countries of the East can only become less poor by saving and investing money in better education, agriculture, social services and industrialization. If they are to do this out of their own resources alone, they must inevitably increase their poverty temporarily.

Several careful investigations have been made of the needs for external financial aid in the form of

long-term loans and gifts to enable all these 'have-not' countries to make a good start on the road to prosperity. It seems that the immediate need is not far from the round figure of £1,000 million a year, that is, £1 a year for each of the 1,000 million inhabitants of these countries. It is difficult to ascertain the amount actually made available for other than defence purposes from the Western nations under the various aid schemes, such as the Colombo Plan, the Colonial Development projects, technical aid through the United Nations, and through various bilateral agreements. Though quite considerable, the amount is almost certainly only a fraction of the above requirement.

Since, of all the former dependent countries, India has the best worked-out plan of social advance and, moreover, is much the largest, it will be convenient to discuss her situation in some detail. Many of the problems facing India are common to other underdeveloped countries, whether in South-East Asia, Africa or South America. The Planning Commission in New Delhi is aided by a powerful statistical and economic group in Calcutta, presided over by a mathematical statistician, Prof. P. C. Mahalanobis, Fellow of the Royal Society of London. This research group has been assisted by economists from both Western and Soviet countries in the study of the problem of how India can most easily, quickly and painlessly emerge from the nearly static and essentially pre-industrial state in which she found herself on attaining independence in 1947.

During the period of the first Indian five-year plan ending in 1956, the national income rose by 18 per cent. This is an important, even if precarious, achievement, in marked contrast to the stagnation of the previous decades. In the second five-year period, 1956–61, a rise of national income of 25 per cent is planned, that is, 5 per cent a year. Whether India can achieve this without additional external financial aid is now doubtful, especially in view of her present large deficiency in her balance of payments. The plan involves not only attaining the high rate of new productive investment of at least 10 per cent of the national income, but also an annual return of 50 per cent in terms of additional output on the capital invested. Such a high net marginal return on capital can only result from very wise planning and efficient execution of the overall pattern of development. Allowing for the population rise of 1·3 per cent a year, the planned rise of income a head is 5 per cent minus 1·3 per cent, or 3·7 per cent a year.

Such a rate of rise, if achieved, would be nearly twice the average of Western Europe at the present time. So the relative gap, that is, the ratio between the living standards of India and of Europe, would slowly begin to close. But how slowly! If these rates continued, nearly half a century would elapse before the living standard of India would climb from one-tenth to one-fifth of that of Europe. Moreover, the actual difference between the living standards, that is, the absolute gap, would still widen; for example, if the assumed rates of advance are maintained for the next ten years, the rise of annual income of a European will be much more than the final income of an Indian.

The limit of India's possible rate of advance is set mainly by the available capital for productive investment, and particularly of foreign exchange to buy production goods from other countries. Failing massive external aid, the advance is limited to what a democratic parliamentary government of a very poor country can raise internally by taxation and loans.

It is clear that the population problem looms especially large to those who are pessimistic about the possibility of rapidly increasing material wealth, including food. Historically the population of Europe in the nineteenth century rose faster than that of Asia; but no population or food problem arose because wealth, including food, rose much faster. The population of the United States to-day is rising faster than that of India. As we have seen, the rise of population in such countries as those of South-East Asia does add, in a defined and calculable way, to the cost and burden of achieving economic progress. However, it is a not uncommon error among Westerners to assume implicitly that no marked economic and agricultural progress in these countries is possible, and so to be led to view them wearing social spectacles through which little can be seen but millions of brown babies.

It is interesting to note that both the Governments of India and China have started to take steps to reduce the population rise. Success in these movements towards reducing the rate of rise of population can only be achieved by education, which itself depends on increasing wealth, and this can only come from industrialization. Moreover, increasing industrialization, as part of a general social advance, is the key to increased efficiency of agriculture and so to a greater food supply. Rural education, transport, power supplies, irrigation, agricultural machinery, fertilizers, pesticides and improved seeds are the essential factors.

IV

What is the historic origin of the astonishing differences to-day between the economic and social conditions of the West and the East? A whole host of hypothetical causes have been advanced: differences in natural ability, climate, health, food, natural resources, religions, social systems or methods of government. To attempt an answer to this problem it is necessary to look deeply into history.

Archæologists and historians have now made it possible to grasp the rough outlines of the history of technology. Since this term means 'the science of the industrial arts', and since it is by these arts that the material goods of life are produced, the history of technology can almost be equated with the history of the material aspects of civilization. With the recent publication of the first two volumes of the "History of Technology", with Charles Singer as editor-in-chief, the layman can easily survey in some detail, though with many gaps, the history of clothes, textiles, jewellery, cities, houses, temples, arms and armour, vehicles, domestic animals, crops, writing, sculpture and pottery, from the emergence more than 5,000 years ago of civilized urban life in the river valleys of the Nile, Euphrates and Indus, up to the present day.

Abundant evidence proves the high level of technological achievement at a very early date. The astonishing building feats of the Egyptians and Mesopotamians are world-famous. Less widely known are the gigantic town-planned cities of northern India, such as Mohenjo Daro which flourished more than 4,000 years ago, with their main drainage

systems, vast granaries, and citizens' houses far better than those lived in by most Indians to-day. Innumerable examples abound in our museums or are illustrated in our histories, demonstrating the astonishing technical triumphs of antiquity. The stern beauty of the 4,000-year-old bronze head of Sargon the Great, or the overpowering grandeur of the 3,000-year-old inner coffin of Tutankhamen, alone suffice to prove that the highest artistry was allied to superb technical skill. The modernity in design of many articles of domestic use in ancient times is most striking. A wooden chair or a manicure set from the Egyptian New Empire of 1000 B.C. would not look out of place in the most up-to-date shop-window to-day, and might well be better made. A high level of domestic culture was by no means restricted to a few monarchs and officials. The cities of Crete, Babylonia and Egypt around 2000 B.C. must have abounded in comfortable and cultivated people who led very pleasant lives, almost as refined and luxurious as prosperous people enjoy to-day. In the material arts of life one is constantly impressed by the absence of many major improvements between the height of the great Middle and Far Eastern Empires and the rise of modern technology in Europe about the eighteenth century A.D. Except for the past two hundred years, Rudyard Kipling was not far wrong when he wrote :

> "We tell these tales, which are strictly true,
> Just by way of convincing you
> How very little, since things was made,
> Anything alters in any one's trade."

Slow and steady progress, of course, occurred in some fields, retrogression in others. Among the most important technological innovations since ancient times were firearms, the magnetic compass, printing with movable type, the horse collar, improvements in wind and water mills, and ocean-going ships. The first three were European medieval developments of much older Chinese discoveries which had never before been exploited fully. They all predated the scientific revolution which only began around 1600, and thus belonged to the ancient craft tradition.

The contribution of China to the history of technology is only now beginning to be revealed, particularly by Joseph Needham and Wang Ling in "Science and Civilization in China". Far Eastern technology appears to have been supreme between 500 B.C. and A.D. 1500. Belief in Europe of the technological superiority of the Far East was indeed widespread from Marco Polo in the thirteenth century to the French missionaries of the eighteenth. The wealth of the Indies was proverbial. Venice—"once did she hold the gorgeous East in fee"—grew rich by trading in textiles, spices, porcelain and jewels.

During the first two hundred years of modern science, from 1600 until 1800, science learnt much from technology, but taught it relatively little. The empirical industrial arts were already so highly developed, and indeed had been so for thousands of years, that systematic science had to develop far before it could improve decisively on pre-scientific technology. Even to-day, the biochemist cannot teach a chef how to prepare a better omelette. Despite the interest of the Royal Society in the 'useful arts', it was not until the last quarter of the eighteenth century that the impact of science on technology began to be decisively important.

The vast developments from the seventeenth century to the present day have been almost exclusively a Western achievement. Though the technological foundations on which Europe built arose mainly in the countries of the Near and Far East, these have scarcely shared as yet in the later scientific and industrial revolution.

To the question why what happened did happen, I do not think there is an agreed answer. Certainly it had nothing to do with any inherent European superiority—indeed, the history of the previous millennia might well have suggested the opposite. Most probably it was differences in social and economic organization which were the decisive factors.

Among the positive factors was certainly the growth after the eleventh century of the city states of Italy, followed by the free cities of northern Europe, where merchant entrepreneurs were free from many of the restrictions of feudal society. Not noble but not poor, these men, much like the Ionians of Thales's time 2,000 years earlier, had a pecuniary interest in exploration and in technical innovation. This positive drive towards technological and social change in Europe might not have been sufficient to produce much more than a temporary spurt of pre-scientific technological advance had it not been for the scientific revolution which in the seventeenth and eighteenth centuries grew out of it.

An essential prerequisite for the scientific revolution was the weakening of the opposition of organized religion to the free play of scientific experimentation and speculation. As R. H. Tawney has shown, by the last half of the seventeenth century Europe was weary of a century and a half of religious strife. Religious tolerance first grew in the merchant cities, where it was found essential for trade. Intellectual tolerance followed in its wake. When the Civil War in Britain ended with the victory of the technologically progressive cities and merchant classes over the still feudally inclined monarchy and aristocracy, Britain was free from restrictions both on scientific thought and on the use of money for profitable and speculative investment. The way was clear for Britain to become the first industrial nation and to usher in our present scientific–technological age. However, even in these favourable circumstances the necessary conditions for the 'take-off' did not exist until the end of the eighteenth century. The parliamentary system of government, which is Britain's proudest political achievement and still a major export, arose in mature form out of this long struggle against privilege and restriction.

One notices that the conquest of Asia and America by Europe was largely completed before the scientific revolution. It was Western superiority in the pre-scientific technology of firearms and ocean-going ships which formed the material basis of the rise of the European empires in Asia and America. Europe was then passing through one of those periods of intense and ruthless energy, both mental and physical, which occur, for complex reasons, from time to time in the history of mankind.

Just when Europe was in a crusading, expansive and aggressive mood, the once great Indian Mogul Empire began to disintegrate, as so many empires have done in the past. Portugal, Holland, France and Britain fought long and bitterly for the spoils. Finally India, Burma and Ceylon fell to Britain, Indonesia to Holland, and Indo-China to France, to be governed from Europe until they acquired their independence after the Second World War. China,

though humiliated and partially occupied, never came completely under Western dominance. The vast Chinese Empire seems to have been too rigid to change of its own accord, but survived mainly because of the bitter rivalries between the Western Powers. Japan alone of Asian nations both accepted Western technology and avoided Western dominance. In fact, for a time she beat the West at her own game by achieving a more rapid rise in income than any West European country before or since, and reached a wealth per head at least as high as that of the poorer countries of Europe. This success had a profound effect in those Asian countries which were struggling at that time to be free from Western dominance.

The impact of the West on some of the static or declining pre-industrial civilizations of Asia brought about certain fundamental changes essential to social advancement, including the weakening of the power of the old feudal aristocracies, the growth of a merchant class, the introduction of Western education, science and medicine, and in some cases the beginnings of a modern industrial and transport system. However, in no Asian country except Japan was the 'take-off' achieved.

It is notoriously hard for Westerners to regard with detachment the long period of European ascendancy in the East. With the fading of the ascendancy, objectivity is becoming easier, and the West has no longer any excuse for not appreciating the Asian point of view. The Indian scholar-diplomat, K. M. Pannikar, in his recent book "Asia and Western Dominance", tells the story of the relations between East and West from Vasco da Gama's landing at Calicut in 1498 to the present day. The reader who meditates on some aspects of the story will be inclined to adapt an old aphorism originally applied nearer home and say 'History is something that Europeans should remember and Asians should forget'. In truth, most Asians have, or politely pretend they have, and this is in no small part due to Britain's freeing of India, Pakistan, Ceylon and Burma after the War: in my view, one of the greatest single acts of British statesmanship of this century. It is, I think, a tribute to both the Indian and British peoples that after decades of bitter political struggle, personal relationships between Indian and British individuals are closer than ever before. It is important, however, to remember that most South and South-East Asian countries attribute much of their present backwardness to Colonial rule. This was a constant theme at the recent conference at Bandoeng of the representatives of 1,500 million coloured people.

V

After 2,000 years of relative stagnation of world technology, Europeans staged a scientific, technological and industrial revolution which has transformed within three centuries, and is still transforming, both the material way of life and the mental outlook of mankind. This astonishing achievement of the peoples of Europe must rank with the greatest events of world history. During the centuries when European scientists, technologists and craftsmen were conquering Nature, European fighting men, missionaries, traders and administrators were conquering the world. By the close of the nineteenth century a hundred million Europeans were ruling some 700 million people in Asia, Africa and America.

Understandably, this double and interrelated triumph of the West over both Nature and the world went to its head. By the last half of the nineteenth century the doctrine of the innate superiority of the white peoples, with its corollary of the inferiority and incapability of coloured people, had become widely accepted in the West. To a great extent, white racialism is economic and administrative in origin. Men tend to feel superior to those whom they rule and to despise those whom they exploit.

What has science to say of the innate mental differences between the different races of mankind? Rather little, it seems—at any rate so far. The physical differences of colour, features and stature are obvious without the aid of scientific study. It may well be that innate differences of mental make-up do exist: but scientific study has failed as yet to establish their existence or nature. Moreover, even if mental and temperamental differences could be demonstrated as innate in such qualities as courage and determination of purpose, physical strength and skill, or in musical, literary, artistic, mathematical and analytical power, between the individuals of different races, the attempt to rate different races in an acceptable general order of merit is foredoomed to failure. For no agreement would be obtainable on the relative weight to be attached to the various abilities; indeed, the various human qualities have different values in different environments.

Even if further studies should show marked differences now between different races, it would be as dangerous to extrapolate too far forward into the future, just as the history of technology has shown it to be dangerous to extrapolate back into even the relatively recent past. The artistic and technical triumphs during the Middle Ages of Peru, Mexico, India, Indo-China and China are a perpetual reminder that great creative ability and immense energy have appeared quite recently among many different peoples, with diverse social systems and religions, and vastly different climates. The grandeur of Cusco, Chichen-Itza, Fatehpur-sikri, Angkor Vat and the Forbidden City in Pekin are permanent memorials of not very distant achievements of the coloured people of the world.

I am convinced that Western pessimism about the possibility of social advance by ex-colonial Asian countries is both unjustified in fact and a serious cause of practical error. It has led the West to overestimate the need of the 'have-not' countries for technical advice from Western experts and to underestimate the need for simple financial help. Admirable as many of the schemes of technical aid have been, particularly those in education, medicine and agriculture, the sending of experts to poor countries without the capital to carry out their plans could be as irritating as to send a trained cook to a family unable to pay the baker. In so far as expert aid by Western technical personnel is needed by an underdeveloped country, it is usually both more efficient and cheaper to provide money for the country itself to hire those whom it needs. Unless followed up by massive financial help, some of the West's present aid programme may yet merit the war-time wisecrack of 'offering all aid short of help'.

What would constitute real help? The figures already given suggest that in addition to likely commercial and government short-term lending, an additional £1,000 million a year is needed as a free gift or as long-term loans from the 400 million rich

Westerners to the 1,000 million Asians, Africans and South Americans in the under-developed countries outside the Soviet orbit. To the Western donors, this would amount to a levy of less than 1 per cent of their income; Britain would pay £150 million a year, and this would only postpone by less than a year the expected rise of 50 per cent in British living standards over the next quarter of a century.

To the recipient country, it would mean £1 a year a head in foreign exchange, that is, a 5 per cent addition to their income of £20 a year. If wisely invested, this should allow the standard of life to rise at rather more than an extra 2 per cent a year. It would therefore make an appreciable contribution to the outstanding problem of the now widening gap.

Such aid to a nation with a well-developed national plan would probably not be needed for more than a limited period, perhaps one or two decades. By then she should have achieved the 'take-off' and be in a state of sustained economic growth, able to dispense with outside aid. Extending the aeronautical metaphor, the suggested Western action would amount to using its great wealth and material power to give the 'have-not' countries an 'assisted take-off'. It must always be remembered that without a strong and intelligently directed internal drive for social betterment, no country can achieve much. Outside aid can only help fully those who are already prepared to help themselves to the limit of their capacity.

Some may think I have taken a too severely economic attitude to the problem of the 'have-not' countries and have neglected the more spiritual factors. Undoubtedly the citizens of a static pre-industrial country may possess many qualities such as contentment, humility, kindness and gaiety in greater measure than those of a rich and progressive country. Though this is true, it is a doctrine which the restless and prodigal West might well preach to itself rather than to others. With nations, as with individuals, the ultimate hypocrisy of the rich is to preach the virtue of poverty to the poor.

VI

The essential fact of the present-day non-Soviet world is that the huge gap of material living standards between the industrial and the pre-industrial countries is steadily widening. Moreover, most new technological advances tend to widen it still further. If present tendencies were to continue for many decades, then one would find many of the ex-colonial and similar countries still largely pre-industrial and so very poor. On the other hand, the vast acceleration of technological progress in the West will be leading the most advanced of the industrial countries to pass progressively, if they so wish, through the phase of the four-day week, on the road perhaps to the final bliss, or ultimate boredom—according to temperament—of the five-day week-end. The difference to-day in material wealth cannot be primarily attributed to differences of national or racial aptitudes, but has complex historical origins. Moreover, the gap in wealth is now so great that an important contribution to narrowing it could be achieved at a relatively small cost to the rich industrial countries.

In turning to consider whether the Western world should make this sacrifice, we leave the field of social analysis and enter that of political and moral controversy. Some people will stress the long-range political dangers of the widening gap and that the survival of parliamentary systems of government in many of the under-developed countries may be dependent on the achievement of rapid social progress. Others will lay most weight on military considerations, arising out of the polarization of so much of the world into Soviet and anti-Soviet blocs. If the West does not help, perhaps the Soviet bloc will. This has hitherto been by far the strongest stimulus to what Western aid has been forthcoming. Strong economic arguments can be produced: poor countries are also poor markets for manufactured goods, and may be unreliable suppliers of essential raw materials: such 'enlightened self-interest' is particularly relevant for Britain with her essential dependence on overseas trade. Some of the strongest motives for a substantial aid programme arise from moral and religious values. Many feel that such large differences of wealth, health and opportunity are wrong, and that to work to reduce them would give a new sense of moral purpose to the West. Recently Ritchie Calder has aptly suggested that massive Western aid to Asia might be considered as reverse lend-lease for the incalculable debt which the West owes to the East for the age-old gift of the great tradition of empirical technology which was the essential basis of Western triumphs. One might add—for it is often forgotten—that all the great religions of the world are of Asian origin.

Here are motives enough for each of us to weigh and make his choice. My own view is clear. I think the West should make the great experiment of sacrificing some of its immediate prosperity to give massive aid to the 'have-not' countries. To give this aid would not, I believe, prove very painful and it may do us a power of immediate moral good and, ultimately, of material good as well. If the major nations of the West cannot agree quickly to provide the required £1,000 million, then I would hope that Britain would 'go it alone' and make her contribution of £150 million a year available to her former Colonies, in addition to what she is already doing. In holding this view, I do not forget the many economic and social problems nearer home which need urgent attention.

Scientists and technologists have a special responsibility in this matter, since it is their genius and their skill which alone can bring the material basis of happiness within the reach of all. The progress of the natural sciences, the West's greatest achievement, has been based on experiment. Let us make now this great social experiment to spread the benefits of our labours. At present they reach only a few. A 'have-not' country, bound like a modern Tantalus by the chains of its lack of capital, gazes with unquenchable thirst on the growing riches of modern technology which it cannot enjoy.

Times and problems have changed since Humphry Davy commented so shrewdly on the social state of Britain at the very start of the industrial revolution. I will end this lay sermon—for this is what this address has become—by attempting to transmute his words to suit the contemporary scene:

The uneven division of power and wealth, the wide differences of health and comfort among the nations of mankind, are the sources of discord in the modern world, its major challenge and, unrelieved, its moral doom.

SUMMARIES OF ADDRESSES OF PRESIDENTS OF SECTIONS

STELLAR ENERGY AND STELLAR EVOLUTION

PROF. T. G. COWLING, in his presidential address to Section A (Mathematics and Physics), points out that, until a few years ago, ideas about stellar evolution had to be regarded as mainly speculative. Nearly every possible direction for stellar evolution has had its supporters. Some supposed that massive stars steadily lose mass, and evolve into less-massive forms; others that small stars can grow into massive ones by accreting material. Some thought that giant stars were newly created, and still in the process of gravitational contraction; others that they were stars which have been blown up from smaller radii during their evolution. All these suggestions rested to some extent on observation, and a fresh idea had to be imported before one could distinguish between them.

This idea has been provided by the recognition of the mode of generation of stellar energy. Nuclear processes provide the only source of energy adequate to keep the stars going; moreover, one type of nuclear fusion, in which hydrogen is built up into helium, can function sufficiently fast at temperatures like those met in the central cores of stars (15–25 million deg. K., say) to be able to maintain stellar radiation. Two helium-building processes are important. The first is the carbon–nitrogen cycle, in which a carbon-12 nucleus captures protons and is converted successively into carbon-13, nitrogen-14 and nitrogen-15; a final capture of a proton leads to a fission yielding the original carbon-12 nucleus and a helium nucleus. The second is the proton–proton process, in which protons are built direct into helium nuclei through steps involving first the production of deuterons and helium-3 nuclei, and then the combination of two helium-3 nuclei to form a helium-4 nucleus and two protons. The first reaction is probably dominant in stars appreciably more massive than the Sun, the second in the Sun and all less massive stars. Stars which have exhausted nearly all their hydrogen can generate energy by burning helium into carbon at a temperature of about 150 million deg. K., but this provides only one-tenth as much energy as helium-building.

Helium-building cannot maintain the radiation of stars 1,000 times as bright as the Sun for more than a small fraction of the time that the solar system is known to have existed. Thus among very bright stars obvious signs of evolutionary changes should be detectable. This agrees with recent observations of globular clusters, which are aggregates born even earlier than the Sun. These possess no very bright blue stars; those initially present have burnt themselves out long ago. The brightest stars in them are greatly distended red giants; such giants accordingly are stars well advanced in evolution, which have burnt much of their hydrogen. This conclusion is confirmed by the theory of stellar structure, which can explain the enormous radius of a red giant only if it has a core the molecular weight of which is greater than that of the rest of the star—a core where all the hydrogen has been burnt into helium.

After the red giant stage stars probably contract again. The stellar graveyard is usually supposed to be peopled by the enormously dense but very faint white dwarf stars. Before they can reach this state, massive stars must lose a large part of their mass, either by continuous ejection or explosively. This part of the course of evolution is not yet fully understood. But we are, generally, in the exciting stage of fresh discovery; speculation is being replaced by established fact.

CHEMICAL APPROACHES TO THE INVESTIGATION OF LUNG CANCER

IN his presidential address to Section B (Chemistry), Dr. J. W. Cook directs attention to the very large increase in lung cancer over the past few decades, and to the close statistical relationship which has been established in a number of investigations between the incidence of the disease and the smoking of tobacco—particularly in the form of cigarettes. He points out, however, that there is a legitimate difference of opinion on the interpretation of this statistical evidence. Some authorities believe that the relationship is a causal one; others consider that a causal connexion has not been established and that other factors, such as atmospheric pollution, industrial hazards and even psychological trends among smokers, are important. A decision between the various points of view would probably be best reached by a direct experimental approach to the problem, and it is this consideration which lies behind investigations of tobacco smoke which are now being carried out.

Attempts are being made to demonstrate the carcinogenic activity of tobacco tar experimentally. With cigarette tar produced in a machine designed to simulate human smoking, Wynder and his colleagues have succeeded in obtaining malignant skin tumours in mice and also in rabbits, and Wynder and Wright later found that the active material is contained in the neutral fraction. The tar is evidently a very weak carcinogen, for the experiments had to be continued for many months before tumours arose, and if it was diluted only two or three times, no tumours were obtained.

The active components of the tar have not yet been definitely identified. The known carcinogen, 3 : 4-benzpyrene, has been detected, but the amount found (1–2 parts per million) is considered too small to account for the activity of the tar. Another carcinogenic hydrocarbon found in the neutral fraction is 3 : 4–9 : 10-dibenzpyrene. Another suspect, arsenic, has been found in varying, but always very small, quantities.

Other constituents already recognized in tobacco smoke include hydrocarbons, both aliphatic and aromatic, alcohols and phenols, aldehydes and ketones, acids, nitrogenous compounds including nicotine and its degradation products, and inorganic constituents. The paraffin, *n*-hentriacontane, has been found in smoke and in tobacco itself, and it has

been suggested that this hydrocarbon and others like it present in the tobacco wax form the main source of the aromatic hydrocarbons in the smoke.

Polycyclic aromatic hydrocarbons occluded in the soot in the atmosphere have also been considered as a cause of the increasing incidence of lung cancer, either acting alone or in conjunction with cigarette smoke. Goulden and Tipler estimated about 300 mgm. of benzpyrene per kilogram of soot. Polycyclic aromatic hydrocarbons including 3 : 4-benzpyrene have also been detected in the exhausts of internal combustion and diesel engines. Again, Kotin, Falk and Thomas, in Los Angeles, have obtained tumours on mouse skin with artificially produced smog free from aromatic hydrocarbons. There is some evidence that the responsible agents here are epoxides arising from oxidation of hydrocarbons in petrol vapour.

There is little doubt that specific chemical substances are also responsible for the increased liability to lung cancer found among workers in a number of industrial processes. The classical example of this, among the miners of Schneeberg, in Saxony, and of Joachimsthal, in Bohemia, is almost certainly due to ionizing radiations from radioactive gases and/or dusts. Other examples, in the nickel, chromate and asbestos industries and among the hæmatite miners of Cumberland, seem to be associated with dusts of metals or metallic oxides or salts. The total number of cases attributable to these occupational causes may be very large, and Hueper, in fact, regards such causes as outweighing cigarette smoking in importance.

Clearly, the whole question of lung cancer and its increase is complex and is unlikely to admit of a simple solution. The balance of evidence is strongly in favour of the view that carcinogenic chemical agents are primarily concerned, although in the present state of our knowledge, it is very difficult to arrange the sources of these in any undoubted order of importance.

ORDOVICIAN VOLCANOES

THE presidential address by Dr. G. H. Mitchell to Section C (Geology) recalls the centenary this year of the birth of a great Irishman, John Joly, to whose brilliant thought modern ideas of vulcanicity and earth-movement owe so much. Joly suggested that a direct connexion exists between heat accumulated within the crust of the Earth as a result of radioactivity and the periods of orogenesis and volcanic outbursts which recur from time to time.

Despite the vicissitudes of many millions of years of geological history, there appear even now among the outcrops of Ordovician strata at the surface in the British Isles a remarkable number of volcanic rocks which afford evidence of one of those outbursts of volcanism. They form, as it were, a great broken ring, near the centre of which lies the city of Dublin. In Ireland the most important exposures are in Tyrone, Galway, Waterford and Wexford. Across the Irish Sea, the Welsh Ordovician rocks, stretching from St. David's to the Menai Straits, include numerous lava-flows and pyroclastic rocks. Farther north, the products of great volcanoes form most of the high fells of the English Lake District. Other remnants of the eruptions of those far-off days are found across the Scottish border in Ayrshire, as scattered outcrops throughout the Southern Uplands of Scotland and even as far afield as Arran, Aberfoyle and Stonehaven. We do not know how many more such deposits lie buried beneath later rocks or have been removed by erosion during the many periods of denudation which have occurred within the 350–400 million years that have elapsed since they were erupted.

The composition of the lavas varies from andesites to rhyolites, with notable developments of spilites. The pyroclastic rocks range from agglomerates to the finest dusts, and include lithic, crystal, vitric and welded tuffs, comparable in composition to the lavas with which they are associated. Both lavas and tuffs show considerable alteration, with the formation of many secondary minerals.

Ordovician volcanic activity broke out early in Arenig times at a number of centres in Ireland, Wales and Scotland. So far as the imperfection of the geological record will allow, the violence of the eruptions appears to have increased with the opening of new foci in the Llanvirn Epoch, in particular in the English Lake District. There is evidence that this phase was followed by uplift and considerable erosion, which are revealed by striking unconformity, pre-Caradoc in age.

Volcanic activity flared up again in Caradoc times. To judge by the thickness of the surviving deposits, there must have been centres of importance in Ireland and North Wales, but in England and Scotland the outpourings were on a smaller scale. With the passing of the Caradoc Epoch volcanism waned, and in the Ashgill Beds, formed at the close of the Ordovician Period, there only remain thin beds of tuff or lava to mark the dying phases of one of the most violent of those volcanic storms which on several occasions have swept the area where the British Isles now stand.

EVOLUTION OF ORGANIZATION IN THE NERVOUS SYSTEM

PROF. J. Z. YOUNG points out in his presidential address to Section D (Zoology) that the controlling functions of the nervous system are a special development of the systems by which all life is regulated. The properties of signalling, memory and prediction that we attribute to the nervous system can also be recognized throughout cellular activity. In order to describe complicated self-adapting systems such as living organisms, we have to consider that there is a sense in which the system 'could' do several things and that it 'selects' the appropriate ones in the light of past history. The behaviour of such complicated aggregates can only be adequately described if we know their history over considerable periods. One method for handling such a study is to use the analogy of signalling and language, where the function of a symbol is to influence the choice among the various things that a human being can do. The significance of the word in this respect is not seen from the wave-form of its sound unless the student has a complete history of the way this wave-form has influenced human populations in the past.

For the control system of an organism to be able to maintain stability it must contain what we may call a coded representation of those events in the outside world that are likely to be significant for its life. We are familiar with this concept when con-

sidering the storage of memories within the nervous system, but it is equally applicable to the control systems of cells. The replication of a representation without distortion through a series of copies requires that it shall consist of discrete units. The molecular aggregates within cells can be considered as such specific unit signals, not only within the nucleus but probably also in the cytoplasm.

Signalling between cells presumably developed out of the system of signals within cells. As nervous systems became more complicated they allowed selection of responses from larger sets of possible actions. Moreover, the actions are influenced by memories of sets of occurrences in the past. In *Octopus*, memories can readily be set up ensuring that one figure will be attacked and another avoided. If the amount of tissue available is reduced by removal of part or all of the vertical lobes, there is a corresponding reduction in the accuracy with which the animal forecasts the probable outcome of a situation. The nervous tissues responsible for the storage of the memories consist of a set of lobes each connected with the next through a plexiform arrangement and containing numerous small cells. The lobes can re-excite each other in a circuit. The specific representations that ensure correct responses presumably consist of facilitated pathways in these networks. During the period after an octopus has received food, the probability that it will attack any moving figure is increased, and conversely after a shock. These effects still occur after removal of the vertical lobes, but both last for a shorter time than in the normal animal. In the absence of these lobes, octopuses can learn only if trials are given at short intervals. Moreover, memories set up before operation are disturbed but not wholly lost after removal.

Although we are still ignorant of the nature of these memory processes, we can see that they involve selection among many possible alternative pathways, such as are open to a system composed of aggregates of dissimilar entities. Such systems can show the process of 'adaptation' with various time-scales, either by selection of certain responses during the life-time of the individual or by the slower process of 'natural selection'. In general, animals with large brains that learn well live longer, reach a larger size and breed less rapidly than those which depend mainly on natural selection to ensure that their responses are adequate. All these problems can be considered together. We can inquire how each species receives the information that ensures that its control system contains a representation that elicits responses which ensure survival. The analogy of information theory is helpful at least for introducing us to the nature of the problems to be solved.

THE EVERLASTING HILLS

"MOUNTAINS are not everlasting, nor are they by any means all of the same age. The antiquity of the Alps and the Himalayas is little compared to that of our Scottish and Welsh mountains, which indeed are so low because they are so old." This view, expressed by Lord Avebury in 1902, is quoted by Prof. David L. Linton in his presidential address to Section E (Geography), who points out that it is quite in line with that of Sir Archibald Geikie, who thought the "prodigious denudation" involved in cutting the glens of the Scottish Highlands could be ascribed to Tertiary times, but relegated the fashioning of the "plane of marine denudation" represented by the present Highland summits to the Palæozoic.

In the half century that has since elapsed, it has become clear that mountains can be carved from uplifted segments of the Earth's crust in much less time than such workers supposed. Thus the Kaikoura Ranges of New Zealand, some 9,000 ft. high, have been carved from the solid in Pliocene and later times. The mountains of Scotland erected by the Caledonian orogeny were laid open to their granite cores, and possibly peneplaned, before the Middle Old Red Sandstone was deposited. The Hercynian mountains of Devon which were rising in Lower Coal Measure times had been largely destroyed by early (and completely peneplaned by late) Permian times. From such evidence, combined with the absolute chronology provided by radioactive dating, it becomes clear that high mountains on continental margins are likely to be base-levelled in 20–40 million years; and since post-Cretaceous time is reckoned to comprise some 70 million years; it is evidently more than ample for two erosion cycles to have been completed in Britain, on hard and soft rocks alike. It is clear also that our existing hill and mountain scenery will display no pre-Tertiary elements unless these have been exhumed during Tertiary time.

Geomorphologists in Britain and Ireland are to-day attempting to educe from the forms of our hills the pattern of their physiographic history. In the mountains of most drastic glacial sculpture, which can be shown to be essentially those that to-day receive more than 100 in. precipitation a year, little if anything survives that can be called pre-glacial. Elsewhere, and especially in the massive uplands of eastern and southern Scotland, central Wales and south-east Ireland, the uplands have been only lightly modified by the ice and enough survives from later Tertiary times to make attempts to construct a denudation chronology both possible and profitable. Such attempts are still in the exploratory stage and indeed it appears that some of the work done in the past twenty years has pursued a false scent and led to conclusions for some areas that are inherently improbable. This set-back, however, need not daunt us, and if care is taken to see that all the possibly relevant evidence is used, and to avoid over-dependence on a single set of morphological criteria, there is no reason why the familiar forms and features of the hills of our homeland should not in time disclose their ages to within two or three millions of years.

THE BALANCE-OF-PAYMENTS PROBLEMS OF A EUROPEAN FREE-TRADE AREA

IS it possible in Western Europe to combine free trade with the maintenance of full employment and the avoidance of deficits on international balances of payments? Prof. J. E. Meade, of the London School of Economics and Political Science, states in his presidential address to Section F (Economics) that there are five possible approaches to an answer to this question; they may be called the liquidity approach, the gold-standard approach, the integration approach, the direct-control approach, and the exchange-rate approach.

(1) The *liquidity approach* relies upon increasing the international monetary reserves of the deficit countries. This will not remove a permanent balance-of-payments disequilibrium. But the surplus countries of Europe might provide temporary finance to the deficit countries of Europe to tide over the time during which more basic remedial measures were having their effect. The European Payments Union is not well designed for this purpose; but a European Monetary Fund might be used in its place.

(2) The *gold-standard approach* means that a country with a surplus on its balance of payments (like Germany) must inflate, and a deficit country (like France) must deflate, its domestic prices and incomes until the balance-of-payments disequilibrium is removed. Surplus countries certainly have an international obligation to avoid domestic deflations and deficit countries to avoid domestic inflations. But it is dangerous to rely solely on this method, because it would necessarily mean the abandonment of domestic monetary and budgetary policies for full employment and for domestic stabilization by the national governments of Europe.

(3) The *integration approach* involves a sufficiently extensive unification of European financial policies and institutions so that payments between two European countries, such as Germany and France, present no more difficulty than payments between two regions of one country, such as England and Wales within the United Kingdom. This, however, involves a European government with powers to institute a common currency and central bank, to carry out a single European budgetary policy, to determine a single European commercial and exchange-rate policy *vis-à-vis* third countries and to carry out an effective development policy for depressed regions in Europe.

(4) The *direct-control approach* implies that a deficit country should be free to restrict imports from other countries. If it restricts imports from its partners, this is, of course, a direct contradiction of the idea of a free-trade area. A European deficit country may be able to do something by restricting imports from third countries without restricting imports from its European partners; but in the circumstances of Europe, not too much reliance can be placed on this possibility.

(5) There remains the *exchange-rate approach*, which means that the foreign-exchange value of the currencies of the surplus countries should be raised and of the deficit countries should be lowered.

An effective solution of the balance-of-payments problems of a European free-trade area rests upon a combination of elements from all these five methods. Exchange-rate variations will certainly have to play a larger part than they have in the immediate past. In particular, some appreciable revaluation of the German mark and devaluation of the French franc are essential.

EDUCATION AND TRAINING FOR THE ENGINEERING INDUSTRY

SIR DAVID ANDERSON, director of the Royal College of Science and Technology, Glasgow, takes "Education and Training for the Engineering Industry" as the subject of his presidential address to Section G (Engineering).

Table 1. Output of Professional Engineers

The figures for first degrees and diplomas are those given in the University Grants Committee returns under "Technology"; the great majority of these will be engineers. The London figures all relate to engineering. The Higher National Diplomas are in mechanical and electrical engineering, and the Higher National Certificates are in mechanical, electrical, civil and production engineering. It has been assumed that 50 per cent of those getting Higher National Certificates are of potential professional standard

Qualification	Session 1950–51	1951–52	1952–53	1953–54	1954–55	1955–56
First degrees and diplomas (University Grants Committee returns)	3,175	3,145	2,999	3,009	2,760	2,871
London, internal degrees from institutions with recognized teachers	242	268	231	209	219	233
London external degrees	570	456	469	309	250	237
Higher National Diplomas	245	203	264	239	251	310
Higher National Certificates, 50 per cent of output	2,598	2,687	2,769	2,860	3,041	3,107
Total	6,830	6,759	6,732	6,626	6,521	6,758

Engineering is the most widespread and basic of our manufacturing industries. Not only is it a major industry in its own right but also nearly every other industry depends on engineering for the design, operation and maintenance of its plant and equipment. The output of engineers in Britain lags far behind our requirements. Every scientific discovery opens up a fan of potential applications, but as Table 1 shows, our output of engineers remains relatively static, and, as a result, many promising developments have to be put in cold storage.

It is difficult to say anything new about education and training for the engineering industry, but the efficiency of the industry is so intimately dependent on the quality of both that periodic reviews are justified.

A problem common to all technological courses is the place which should be given to 'liberal studies'. The magnitude of the problem can easily be exaggerated, but there is general agreement that they should have a place. The difficulty usually alleged that there is no time could be overcome by taking some half-hours from the many laboratory periods, and devoting them to non-technical subjects. As regards the use to which the periods might be put, everyone will have his own ideas, and rightly so. There can be a wide range of choice.

Universities and technical colleges, especially technical colleges, are frequently blamed for failing to teach their students how to think; but this criticism is often unfair. On the whole, the quality of teaching is good; the trouble is that good thinkers are rare. Nevertheless, the problem should not be glossed over, and, with the allied problem of overcrowded syllabuses, it might very well be the subject of some 'work study' research by university departments of education and the proposed staff college for teachers, if and when it is set up. For one thing, more departmental co-ordination is needed. No manufacturing firm could run with as little departmental co-ordination as takes place in the average university.

Increasing attention is being paid to management studies in engineering courses. Without good management, the maximum return cannot be reaped from technological advances. An ideal arrangement

would be a planned series of short residential courses spaced throughout the earlier years of a man's professional life.

A higher standard of technology needs to be backed up with better technicians and craftsmen, and British industry should make a serious examination of its apprenticeship system. At the start of the century, apprenticeship was much the same in Continental countries as in Britain; to-day British and Continental practices are widely divergent. In West Germany apprenticeship is controlled on a national basis, with compulsory attendance at a vocational school, and an apprentice must pass a written, oral and practical examination before he can be certified as a skilled craftsman. The scale on which apprentice training is conducted can be seen from figures for the city of Frankfort (population about 600,000), where, in a recent year, 4,800 candidates were examined in 126 different trades. In France an apprenticeship may be served not only in industry but also in an apprentice training centre. There are nearly a thousand such centres catering for 160,000 apprentices. In the light of what is happening abroad, it is disturbing to reflect that, in Britain, a youth can still become a skilled craftsman without having attended a single class or passed a single examination in the whole five years of his apprenticeship—and many do: 'serving time' is the only compulsory requirement.

In the United States people move freely in both directions between industry and teaching. In Britain we are far more static, and one reason is the lack of interchangeability in our superannuation schemes. University teachers are on one scheme, technical college teachers on another, and in industry each firm has its own. A person cannot, as a rule, move from one to the other without incurring some kind of superannuation penalty. What is needed is superannuation interchangeability between universities, technical colleges and industry. This would encourage freer movement of staff, which would be of all-round benefit.

In these days of rapid technological advance, teaching staffs should return to industry to be brought up to date in industrial practice, but how rarely this happens. In the Willis Jackson Report on "The Supply and Training of Teachers for Technical Colleges" figures are given showing that, in 1956, only 34 out of 11,500 teachers in technical colleges returned to industry for periods which might be as little as a few weeks or even days.

The engineering industry should establish many more research fellowships. There are too few opportunities for the pursuit of research beyond the Ph.D. stage, which, after all, is not much more than a training in research methods. The number of such research fellowships in Britain in relation to the size of the country's industrial budget is pitifully small.

ARCHÆOLOGY AS SCIENCE: PURPOSES AND PITFALLS

PROF. C. F. C. HAWKES, in his presidential address to Section H (Anthropology and Archæology) considers the question of what sort of science archæology, which has grown out of the physical sciences of geology and palæontology on one hand and the social sciences of history and ethnology on the other, should be. Prehistoric archæology is often regarded as the purest and best because it is the most scientific, that is, it is based solely on the enumeration and classification of material remains, the study of which involves collaboration with natural scientists. Team-work between archæologists and natural scientists is the biggest single factor now operating, at least in the prehistoric field, to improve the quality and extend the range of archæological research. But the theme of Prof. Hawkes's address is not archæology and science, but archæology as science, and he believes more thought should be given to purposes in archæology and to the possibility of pitfalls ahead. The principles behind archæological work are really only two: first, that people in the past did things for purposes which in most cases can be understood; and secondly, that where different people did the same or similar things and connexion is possible between them, there should be single explanations to cover both or all of them. In so far as these principles are put into practice, archæology may be called scientific: the pitfalls lie in neglecting them.

The prehistoric wing of archæology, having its roots in geology and palæontology as evolutionary natural history, studies man in his place in the biome and his gradual development of the techniques and social organizations to enable him to control his environment. Such primary prehistory is indispensable. The study of the Palæolithic and Mesolithic is essentially world-wide, and primary prehistory continues through the transition from hunter–fisher life to the study of communities that grew vegetable and animal food and developed the stable social groups which are the essential feature of the Neolithic. This study has an essential place among the intellectual possessions which make the modern outlook upon the work differ from any previously attained. The distinctively scientific characters of primary prehistory are that it excavates sites according to the principles of stratigraphy borrowed from geology, it makes exact measurements and comparisons in the classification of finds, and collaborates with the natural scientist in the analysis of anything that will aid archæological interpretation.

Progress in the study of the Palæolithic is satisfactory. Evidence about the economics of Upper Palæolithic hunting life is not wanting; the size of the social group can be estimated in terms of hunting strength and winter house size, and it is possible to follow the seasonal movements following game. Upper Palæolithic art can be interpreted in terms of hunting-magic, and in it are revealed the germs of a religion of animal and human fertility. Much the same is true of the hunting and fishing communities of the Mesolithic. The diffusion of a new mode of subsistence by farming in the Neolithic, and movement across the continents, which in the Palæolithic and Mesolithic are to be explained almost entirely by climatic factors, can in the Neolithic be understood as a search for new land reinforced by the pressure of the growth in population. All this is intelligible to reasoning based wholly on material and environmental factors.

However, European prehistory in the second as well as in the first millennium B.C. cannot justifiably be treated as altogether primary. There is nothing demonstrably climatic nor arguable from the food quest to explain the diffusion of Bell-beakers and their associations over so much of Europe in the second millennium. What explains the proliferation

and diffusion from the south Russian steppe and the east European plain of people burying their dead under round barrows, singly, and with stone shaft-hole axes and pottery embellished with cord-impressions? Here explanation has to pass beyond the primary level of reasoning if it is to remain scientific and adhere to the evidence. In the first half of the second millennium B.C., Indo-European languages were being spoken in Anatolia and Greece. Early Greek is revealed in the Linear *B* Minoan and Mycenæan tablets, and its evidence can be used in conjunction with later Greek evidence, and with reasoning on the lines of comparative philology, to augment the conclusions philologists have already drawn on the antiquity of social and religious institutions common to all the speakers of languages of that Indo-European family.

Wherever archæological studies approach evidence bearing on language or upon institutions known from texts, that is, wherever cultures are found not directly explicable by the economic basis of subsistence, it is necessary to transcend prehistory in the primary mode, and to collaborate with the philologist and historian. This way, too, lies the needful reconciliation between archæology and ethnographic and social anthropology, which require societies to be apprehended as functional wholes. Archæologists must welcome all trustworthy evidence, whether from scientific or from humanistic sources, which will illumine the transition from prehistory to history. The trouble with archæology is that alone it can reveal so little that is non-material.

The claim that archæology should be regarded as a science is bound up with its ability to compare and classify finds and to construct systems on that basis. To-day the periodical literature is so great that it is becoming less and less easy to find published comparisons. The basic information of the natural sciences and of humanistic scholarship is gathered into works of reference. But with rare exceptions archæological books are all introductory or popular. Prof. Hawkes calls for a corpus treatment of all archæological antiquities on uniform lines, not in books but on cards. The "Inventaria Archaeologica" is meant to cover a selection only. It ought to be backed up by card-corpuses which, Prof. Hawkes suggests, might be prepared in a period of five years, for Neolithic, Bronze and Iron Age Britain and Ireland, at a cost of some £10,000. Central institutions in capital cities would be required to keep the corpus annually up to date. Thus the material facts of archæology would be accessible to all and only then, not by faith alone but by works also, could archæology fully substantiate its claim to be a science.

CHEMICAL TRANSMISSION AT NERVE ENDINGS

TRANSMISSION may be defined as the communication of activity from one cell to a second, anatomically separate cell; synaptic transmission occurs where both are nerve cells; neuro-effector transmission where the second is an effector cell. The presidential address given by Sir Lindor Brown to Section I (Physiology and Biochemistry) is limited mainly to neuro-effector transmission. The predominating effector is the skeletal musculature; other important effectors are smooth muscle and gland cells. Both nerve and effector cells constitute closed systems in which, at rest, the inside is 50–90 V. negative to the outside. Both have the property of excitability and the ability to conduct self-propagating waves of electrical activity brought about by a sudden change in the properties of the limiting membrane which permits local electric currents to flow, and excite similar changes in the adjacent membrane. Transmission from one cell to another involves more than this electrical spread. Specific substances are discharged at the nerve endings which excite or depress the next cell.

At the skeletal muscle junction and ganglionic synapse, the chemical transmitter is acetylcholine. In the nerve endings at the motor end-plate there is a large number of units, capable of liberating a small charge of acetylcholine. Individual units are continuously discharging in a random fashion, but the amounts are insufficient to excite. With the arrival of a nerve impulse a hundred or more units discharge synchronously, the acetylcholine released reacts with receptors in the muscle fibre, short-circuits the end-plate membrane; and local electrical currents sufficient to depolarize the adjacent membrane and to initiate a propagated wave of depolarization, are produced. The combination of acetylcholine and receptor is rapid and is limited by the destruction of the transmitter by the enzyme cholinesterase, concentrated at the end-plate. It has even been suggested that this enzyme is the receptor.

Information on synaptic transmission is scantier largely because of the difficulty in recording electrically from the inside of a ganglion cell. The longer duration of transmitter action and the less obvious effect of anticholinesterases suggests that diffusion rather than destruction may be responsible for the removal of the transmitter.

Post-ganglionic neurones of the involuntary nervous system may liberate either acetylcholine or noradrenaline. On the heart, cholinergic (vagal) fibres depress, whereas adrenergic fibres excite. This depression of cardiac muscle by acetylcholine might suggest that its mode of action differs radically from that at the motor end-plate. Recent work suggests a fundamental similarity. At the motor end-plate the membrane permeability to all ions is increased, so that depolarization and excitation result. In the heart, the increase is restricted to the potassium ions and possibly the chloride ions, with a resulting hyperpolarization and reduced excitability. The mode of action of adrenaline and adrenergic nerves on the heart is still obscure.

Information on the action of the chemical transmitters on smooth muscle is also limited because of the difficulty in recording electrically from inside the cell. It appears, however, that where acetylcholine excites, as in the intestine, it also reduces the membrane potential, and that adrenaline, which inhibits, increases the membrane potential.

Although the nerve impulse is 'all or nothing', the amount of transmitter liberated is almost certainly variable. At the muscle end-plate previous moderate activity can increase the number of units released, whereas excessive activity can lead to a decline sufficient to produce, in amphibian muscle, complete neuromuscular block. However, under normal conditions, similar exhaustion of the terminal stores of transmitter is unlikely in post-ganglionic neurones because of the low frequency of natural discharge.

Accumulation at, and diffusion from, the site of liberation of acetylcholine with consequent extension of its action is unlikely. The presence of 'true' cholinesterase at the site of liberation and non-specific cholinesterase in the blood and tissues offers a double barrier to such extension.

Without eserine to protect acetylcholine from enzymatic destruction, little would be known of cholinergic transmission; what little is known about adrenergic transmission has been gleaned without the use of a specific inhibitor of any inactivating enzyme. Mono-amine oxidase has been suggested as playing a part comparable to cholinesterase. There is much evidence to support this, but recent work attributes inactivation to combination with the tissue receptors. Whatever the mechanism, it is easily swamped; so that accumulation of noradrenaline, local extension of its action by diffusion, and escape into the blood stream can all occur. As a consequence, localization of action comparable with acetylcholine-mediated effects is improbable.

THE WORKER AND HIS WORK

IN his presidential address to Section J (Psychology) Dr. H. G. Maule points out that at the beginning of the nineteenth century, as industrialization proceeded with great rapidity, there was but little attention paid to the personal and human needs of the industrial worker. In general, industrialists were concerned more with the new, developing, techniques of engineering and with new machinery than they were with people. There were, however, individual examples of firms which appreciated that workers as much as machines required attention. Against this background a developing public conscience gradually intervened, and laws were passed which helped to make the workers' lot more tolerable. Leonard Horner, one of the original inspectors of factories, reported some interesting and successful experiments in which hours of work in the cotton industry were reduced, with advantage not only to production but also to the general well-being of the worker. He admitted that he had not realized that shorter hours of work could lead to more punctual attendance, the avoidance of waste, and the general raising of industrial morale and so increase industrial efficiency.

Towards the end of the century a carefully conducted experiment on the effect of reduced hours was carried out in a Lancashire engineering works, and the result confirmed these findings of the earlier pioneers. Speaking of this experiment, Sir William Mather said that in his view the most economic production was obtained only so long as workers were at their best, and no economy was effected by continuing beyond this point. Up to 1914, however, it was only in respect of hours of work that there was any serious thought about the relationship between the worker, his work, and his working efficiency. In 1914, faced with war demands, industry attempted to solve its production problems by extending hours of work with little or no limit. So unsuccessful was this policy that a special committee to investigate the health and efficiency of munition workers was set up in 1915. When this committee was disbanded, it was succeeded by a peace-time organization, the Industrial Fatigue Research Board, which for the next thirty years carried out research and published its findings. In this way the foundations for modern industrial psychology were securely laid. At that time the main problems were those of working environment and methods of work. Even so, the early investigators directed attention to many of those aspects of people at work which are to-day regarded as the most important.

Between the two World Wars the emphasis changed. Studies carried out in the United States stressed the great significance of psychological forces operating within the working group and demonstrated the adverse effect on both well-being and production which might result from failure to pay adequate attention to these psychological forces.

By the time of the Second World War a substantial body of knowledge had been accumulated about conditions most favourable to efficient performance. But even so, many managers of industry were ignorant of the knowledge that was available to them.

The war years saw psychology playing its main part in application to the problems of the Armed Forces, particularly in the development of personnel selection. In this field striking contributions were made to the more effective use of Service personnel.

Since the end of the War there have been further important developments in the application of psychological principles to industrial problems, though there is still a considerable gap between knowledge and its application. We know, for example, that industrial neurosis accounts for the loss of some seventy million working days each year. It has been suggested that £700,000,000 a year is lost through people changing from job to job. We know some, but not all, of the factors influencing psychological mal-adjustment; we are beginning to understand the significance of the attitude of the worker not only to his work but also to those with whom he works. Design of equipment and machinery is now being recognized as having a psychological as well as engineering implication. A beginning has thus been made in our knowledge of the psychological needs of people at work.

Industry should recognize that sociology and psychology can make a contribution towards solving the problems of industrial management.

ASPECTS AND PROBLEMS OF CONIFER EMBRYOLOGY

PROF. J. DOYLE remarks in his presidential address to Section K (Botany) that our understanding of the nature of conifer embryology has been somewhat hampered by the fact that early work was centred on north temperate forms with special emphasis on the Scots pine. As a result, some presentations seem to make the matter a little obscure, whereas in reality it is relatively simple. The various embryogenies in the conifers can be largely resolved into a number of obvious modifications of a basal plan, with only a few cases which present any difficulty in fitting into the scheme.

The nature of what appears to be the basal plan can be most readily appreciated from a consideration of embryogeny in the podocarps. After fertilization, the diploid fusion nucleus undergoes a number of free mitoses, up to five, to give at the archegonium base a number of free nuclei, up to 32. At membrane formation these are arranged in a single upper tier of unclosed cells, the 'open' tier, and a lower group of primary embryo cells. An internal division,

affecting all the nuclei, follows. Divisions in the 'open' tier are orientated along the axis of the pro-embryo and thus give rise to an uppermost single tier of cells, the upper or *U* tier, and, just below it, the next single tier of cells, the suspensor tier, *S*. The primary embryo nuclei are also duplicated. The nature of the mature pro-embryo can be expressed as a formula, *U : S : E*, to which can be added in any given case the number of cells in the parts so developed. The *U* tier is most often ephemeral. The *S* tier elongates to give the primary suspensor.

Excluding the araucarians, this plan is found in all the conifer families. In the Pinaceae, however, the *S* tier does not elongate. This is the so-called 'rosette' tier of *Pinus*. The proximal layer of the *E* cells elongates to function as a substitute suspensor. *Pinus* embryogeny, in detail, is not typical of conifers.

The varying embryogenies in the families and genera can readily be related to a limited number of general evolutionary trends the expression of which seems to be, most often, in no way related to systematic affinities. This phenomenon, the expression of tendencies without apparent regard to systematic affinity, is a dominating feature in conifer embryology both in the pro-embryonic and gametophytic phases. As extreme examples there may be cited: the occurrence of four-celled pro-embryos in forms as distinct as *Athrotaxis*, *Callitris*, and *Torreya*; the appearance of similar types of cleavage polyembryony in *Taxodium*, *Keteleeria* and *Podocarpus*; or the hyphal-like pollen tube in *Tsuga*, *Saxe-Gothaea* and *Araucaria*. Embryological data, therefore, can be applied only with great discretion, if at all, to questions of affinity in conifers. If, however, the embryological data be considered in themselves, it is now possible to indicate, at least along general lines, the main evolutionary trends which have resulted in modifications in the basal plan.

Evolution in the conifers thus appears to be related to two groups of evolutionary trends: one group related to systematic divergence and diversification, the other to modifications in embryological stages. These two groups appear to be separate, as it were overlapping, and the nature of their connexion, which must be intimate, remains a problem. Selection does not seem to be a factor affecting the embryological trends.

THE EDUCATION OF THE COUNTRYMAN

IN his presidential address to Section L (Education), Mr. L. R. Missen defines the countrymen he has in mind as the two to two and a half millions in Britain who live in the open countryside outside villages and probably three and a half to four millions who live in villages. He deals with two misconceptions which are still held by the urban population: that the countryman is the mental inferior of the townsman; and that all those who live in the country work on the land. These misconceptions have in some degree affected the recruitment of teachers to rural schools. The evidence which he quotes shows that with regard to the former the case is at least 'not proven', and there is no difficulty in showing a wide variety of occupations in most rural areas. The theory that country children are mentally inferior to town children is examined, and the evidence of Sir Godfrey Thompson and others is brought to show that this is a false idea. On the other hand, there are distinct differences between rural children and town children. It is claimed that whereas the country child is frequently slower than his counterpart in the town to grasp a new idea and to assimilate facts, he is more thorough and more painstaking. The country child may lack the fluency of the urban child, but fundamentally his mental equipment is just as good.

The rural school child has been handicapped in the past in many instances through education in schools which were too small, although small schools have some advantages over larger ones. The single-teacher school is rapidly disappearing and the two-teacher school will be the smallest.

In the life of the countryside the most remarkable change is coming about through the mechanization of agriculture, and there is now a pressing demand for men and boys with a sound knowledge of the elementary principles of science. At the school-level, the most exciting development is the new secondary modern school, which is providing an increasing number of well-trained and competent men and women. The rural grammar school will continue to make a valuable contribution in the country-side.

The methods of agriculture, now increasingly scientific, are being met in several ways. The National Agricultural Advisory Service and the farm institutes have done much to increase the farmer's and the farm worker's understanding of their craft; evening classes of the local education authority and day-release classes have supplemented this work. The countryman is in some ways more fortunate than the town dweller in his choice of leisure time occupations; he is less likely to spend his spare time being entertained at the cinema or at football matches. He is a good reader, and the evening institutes offer him a wide variety of cultural and recreational activities.

Mr. Missen concludes that the countryman has been less impressed by the glamour of the twentieth century, and that the steady and unalterable succession of the seasons will maintain a slower tempo of life, which must have nationally a balancing effect. The countryman has far more time for consideration of life's problems than the townsman.

THE ROLE OF AGRICULTURE IN THE BRITISH ECONOMY

SIR JAMES TURNER, president of the National Farmers' Union of England and Wales, is presiding over Section M (Agriculture), and the theme of his presidential address is "a full measure of technical progress in the future will depend very much upon the agricultural industry being able to count upon the right basis of political and economic conditions".

After referring to the long-term planning that is necessary in agricultural production, Sir James discusses the advantages resulting from a stable industry: "The gain to producers from stable markets is fairly obvious. To consumers and tax-payers, the benefits are complementary, accruing from the avoidance either of sharp changes in the cost of living with their consequent impact on wages and

production costs or of sudden variation in Exchequer liability".

Sir James stresses that the industry's ability to lower unit costs depends not solely upon its own efforts but also upon two extraneous factors. The first of these is ability of the Government to halt inflation; and the second is the efficiency of those industries which supply agriculture with goods and services—for these are valued at the present time at some £800 million a year.

The challenge is to find new and better ways of putting the productive resources of agriculture to the most economic use. First of all, there is no doubt that grass, the United Kingdom's largest and most important crop, offers great scope for rapid development in yield, utilization and conservation.

Work-study can make many contributions to agricultural efficiency. The stage has now been reached when work-study knowledge and advice should be made available to farmers on a basis similar to that of the National Agricultural Advisory Service, and he thinks that every district officer of the Advisory Service should receive training in work-study.

A new interest in farm building design among architects, builders and industrial designers is also needed. That is why the National Farmers' Union of England and Wales has recently established a research fellowship in the design of farm buildings, which is to be administered by the Nuffield Foundation.

On the question of efficiency, Sir James thinks it a pity that whereas farmers are always under pressure on the score of efficiency, very little is said about the efficiency of the ancillary industries. They have the benefit of a firmly based home market, and they should share with the farmers the obligation to cut at least their operational costs to the minimum and so allow the consumer, through the medium of the farmer, to share in that benefit.

As regards the achievements of the producer-marketing movement in Britain, there is no doubt that it ranks, with the guarantee system, as one of the two most revolutionary agricultural developments of our generation. On the distributive side it is in the sphere of presentation, pre-packaging and self-service distribution of branded British foodstuffs, effectively advertised, and carrying a continuing guarantee of quality, that early and dramatic advances can be achieved.

Summing up, Sir James believes that what we really need is a combined operation in the economic, financial, technical and scientific fields if we are to succeed in transforming the structural basis of the industry during the next generation, on a scale comparable to the agrarian revolution of the eighteenth century. With a stable support structure, and a proper flow of credit, there will be unlimited scope for the application of new knowledge.

THE VASCULUM AND THE MICROTOME

IN his presidential address to Section X (Assembly of Corresponding Societies), Prof. D. A. Webb discusses, under the title of "The Vasculum and the Microtome", some of the problems in the relationship between laboratory biology and natural history to-day. The contrast between them to some extent coincides with the contrast between the interests of the amateur and of the professional, but the two distinctions are not quite the same; for although the amateur laboratory worker has unfortunately become almost extinct on account of the increasing complexity and cost of apparatus, there are a number of professional biologists who are primarily field-workers. These men are in an especially favourable position for bridging the gap between amateur and professional and for encouraging each to help the other.

The welcome revival of interest in natural history among professional biologists in the past twenty years, and the many examples of fruitful collaboration that have been seen in this period between amateur and professional and between laboratory worker and field-man, must not blind us to the fact that there is a necessary tension between the two approaches, and that unless each side is able to show tolerance, and respect for interests that it does not share, this tension will find expression in hostility and polemics. Most biologists belong to one or other of two contrasted types, which differ from each other in their primary interest in the subject, and consequently in their method of approach and their sense of what is important and what trivial. One is attracted primarily by the richness and diversity of Nature; he seeks for classifications, principles and laws to help him to explore this diversity effectively and to comprehend it in his mind; but its complexity is for him a primary datum which no theorizing can reduce to simplicity. Such a man is naturally a field-worker. To the other type this complexity and diversity is, if anything, an irritant; he is interested in analysis, abstraction and simplification; he seeks above all for the understanding of the processes of life and of the principles of homogeneity that underlie the diversity. For these ends the laboratory is more effective than the field.

Neither of these attitudes can, if pressed to its logical conclusion and pursued to the exclusion of the other, get very far towards the understanding of the organic world. The complexity of this world is so great that without analysis and simplification we can but contemplate it, wrapped in a fascination that may be pleasurable but scarcely scientific. On the other hand, it must be realized that the organism lives every moment of its life not in the cold serene world of genetic ratios, enzyme equilibria and the thermodynamics of infinitely dilute solutions, but in a grosser, pullulating, phenotypic world, characterized by a complexity of which only a minute fraction is reducible by our laws and theories.

The impact of cytogenetics on taxonomy serves to exemplify both the profit to be gained by close collaboration between the laboratory and the field-worker, and also the dangers of misunderstanding between them. Partly because of the lack of suitable semi-popular expositions of the lessons that traditional taxonomy must learn from cytogenetics, many amateurs are still impatient and distrustful of the subject, and do not realize how often it gives them a clue to unravel a confused situation, or an objective instead of a speculative basis for their conclusions. On the other hand, some cytogeneticists, notably Prof. C. D. Darlington in his "Chromosome Botany", are so impressed with the importance of their own studies that they represent them as the only road to biological truth, and with a fine disregard for logic

hold up to contumely the principles of a taxonomy the categories of which they are using every day.

But in spite of some arrogance on one side and ignorance on the other, a great deal of effective co-operation has been achieved. It is only the rare mind that can at once retain in view the complex pattern of Nature and achieve the abstraction necessary for its analysis; but on such minds we are dependent for any real advance in our understanding of organisms. The rest of us can, however, at least avoid putting obstacles in their path; and in bringing our brick to be built into the structure of scientific knowledge we must realize that our neighbour's brick, though it differs in shape and colour, may be just as useful as our own.

THE REPUBLIC OF IRELAND

A View of Ireland

Edited by James Meenan and David A. Webb. Pp. xvi + 254 + 17 plates. (Dublin: Local Executive Committee of the British Association for the Advancement of Science, 1957.)

THIS Handbook, published especially for members of the British Association by the Local Executive Committee in Dublin, maintains the high standard, especially in sumptuousness of production, which has recently been attained by previous local committees at other meeting centres.

As would be expected, this work concentrates, in twelve chapters, mainly on the geography and geology, biology, agriculture, archæology and folk-lore, and economics of the Republic of Ireland. One chapter deals with the Irish language, and the final chapter is concerned with interesting details about Dublin itself, including its main scientific institutions. In a folder attached to the back cover is a simple map of Ireland indicating the counties and the main towns. The indication of the border separating the six counties comprising Northern Ireland is by no means distinct enough. Many visitors will wish to know which are the six counties, but few will recognize the border on this map. The fact is that the border is there, and should be clear on such a map. A second map will prove more useful since it gives details of Dublin and the adjoining counties. There are seventeen well-produced plates and twenty-two maps and diagrams interspersed in the text.

As the title implies, this Handbook ranges over the whole of the Republic and does not concentrate on Dublin, the venue of the meeting, and its immediate surroundings. In this respect the book differs from its predecessors; but nevertheless the editors made a wise decision in this connexion. Some chapters, for example, physiography, geology and botany, range over the whole of Ireland (including Northern); others are confined to either the whole or just a part of the area south of the border.

Mr. J. P. Haughton has written a valuable survey of the physiography and climate of the entire island. Geology is treated by three authorities: Prof. J. C. Brindley on basement rocks; Prof. W. D. Gill on the Upper Palæozoic Systems; and Mr. G. F. Mitchell on the Pleistocene Epoch. Prof. D. A. Webb deals with the terrestrial flora, and Dr. M. de Valera and Miss H. M. Parkes describe the rich algal flora of the east and west coasts. The zoology of Ireland is so vast that in tackling this section Prof. C. F. Humphries and the Very Rev. P. G. Kennedy have wisely confined themselves mainly to the south-east. Inland fisheries loom large in the economy and sport of Ireland, so a separate chapter, by Dr. A. E. J. Went, deals with these. Thirteen families of fish are to be found in the fresh waters, the most important from the economic point of view being salmon, sea-trout and eels.

Prof. G. O'Brien writes on the economic structure of the Republic.

Agriculture, Ireland's main industry, is dealt with by three authors: Dr. J. J. Byrne, who presents a general survey; Dr. R. O'Connor, who writes about the small farms which abound there; and Dr. D. Twomey, who describes the important livestock industry. Peat is an important fuel in Ireland, so it is fitting that a chapter should be devoted to it in such a book as this. It was compiled for the Irish Peat Board by Mr. H. M. S. Miller, and is of special interest in dealing with peat winning, preparation and artificial dehydration.

The archæology of the Republic covers a wide field. Prof. S. P. Ó. Ríordáin deals with the prehistoric period, and Dr. F. Henry the early Christian period. Local traditions in Ireland are exceptionally fascinating, and still offer wide fields for research. Prof. J. H. Delargy describes the folk-lore in general; place-names are considered by Dr. M. A. O'Brien; and Mr. A. T. Lucas discusses folk-life. Prof. M. Dillon's excellent chapter on the Irish language as one of the Celtic group seems somewhat out of place in such a Handbook as this because most of the Association's visitors will have little time for studying it even cursorily, whereas any serious student would certainly wish for more.

The City of Dublin is covered in the last chapter by Dr. R. B. McDowell, who describes its growth; Mr. J. Meenan, who considers its role in the Irish economy; and finally by the two editors of the Handbook, Mr. J. Meenan and Prof. D. A. Webb, who present a useful, but all-too-short, account of the principal scientific institutions in Dublin, including the universities and colleges, learned societies, museums and botanic garden, etc.

Each chapter of the Handbook is followed by a very useful bibliography. It is a pity that there is no account of the development and present position of education in Ireland. But the most serious omission is that of an index. A Handbook of this sort, though handsome in appearance and making good arm-chair reading, is essentially a work of reference; and such a work must have an index if it is to satisfy its readers.

Taunton College School was still called the Free Grammar School.

In 1864, Queen Victoria appointed Lord Taunton as chairman of a Public Schools Inquiry Commission. At this time Lord Taunton was also president of the Board of Governors of King's. He called Tuckwell to give evidence before the Commission concerning the teaching of physical science at his school. In giving evidence, Tuckwell satisfied the Commissioners that a scientific education gave his pupils inestimable advantages in later life.

Tuckwell met with much adverse comment, and although he may not have realized it, his chief obstacle to teaching science in his school was that, in order to move the school to its present premises, a joint-stock company with a governing body had been formed. No longer was Tuckwell free, as he had been under the former constitution of the governing body. Under the old system, the headmaster had received an endowment, which was not accompanied by any sort of restrictions or obligations as regards his teaching or conduct. The duties of the trustees had been to appoint a headmaster, not to maintain the school. His governors grew to dislike this arrangement, with the College School "with a classical side trying to lead in the new science teaching". They disliked as headmaster "a pioneer in the teaching of science and a brilliant exponent of it against the old specialisation in classics". In the face of this, Tuckwell resigned at the end of 1877.

In 1880 the school was acquired by the Woodard Corporation and from that date until recently no startling advance was made; science teaching was continued, but no new facilities were introduced to keep pace with the rapidly expanding school.

However, in the late 1930's it was decided to build a new science block; plans were drawn up; materials ordered and work was due to start in October 1939; but the project had to be temporarily postponed. It was, however, necessary to have additional laboratory space of some description, and so a temporary wooden laboratory was erected, Tuckwell's old laboratory then being given over to the teaching of biology. This new laboratory served its purpose admirably, and in 1943 another of these laboratories was acquired, this being used for the teaching of physics.

After the War, a memorial fund was opened, and in 1947 it was decided that the war memorial and science block should be incorporated in the one building. In 1955, Tuckwell's old laboratory was pulled down to make way for the erection of the new block. When the building was started, £8,000 had been given to the fund, and £20,000 had been lent, by relatives of the bereaved and by friends of the school.

When first designed, the block was intended to consist of two chemistry and two physics laboratories only. However, in 1956 the Industrial Fund for the Advancement of Scientific Education in Schools gave the school a grant of £10,000, and so made it possible to extend the block to include two preparation rooms and a science library, and to fit the laboratories with modern equipment and apparatus. It is interesting to notice the difference in the floor space of laboratories; the new block has a total floor space of 6,000 sq. ft. as compared with the 300 sq. ft. of Tuckwell's old laboratory. The only reminder of the great pioneer who did so much for King's is a plaque set in the wall of the new block, approximately over the site of Tuckwell's laboratory.

GENETICAL HAZARDS OF RADIATIONS*

A YEAR ago I reviewed in *Nature* a report on the biological dangers of nuclear and applied radiations which had been prepared by the Medical Research Council and published by the Government. I concluded with an urgent appeal for an easily readable digest of this important document. However, no such help has been given to the public, which is getting more and more anxious to understand the position. Instead of this, there has been a plethora of statements by scientists which, unfortunately, has probably contributed more to the confusion than to the enlightenment of the public. For the technically trained reader it is easy to see that all these statements agree on certain fundamental facts and conclusions, but the emphasis is placed in such different ways that the general reader may well feel it is more or less a matter of choice or philosophy whether ionizing radiations are considered dangerous or not. Much of this confusion could be dispelled if a recent popular lecture by Dr. Warren Weaver, of the Rockefeller Institute, could be made available to the broadest possible public at the lowest possible costs.

In ten lucidly written pages, Dr. Weaver makes the reader acquainted with the indispensable scientific background to the problem of radiation hazards. He limits himself almost entirely to genetical hazards; the dangers to the immediately exposed persons are scarcely considered. It is true that this leaves out of consideration just those effects—like bone cancer caused by strontium-90 or leukæmia caused by diagnostic X-rays—which at the moment cause most concern. But these are also the dangers which can be most easily understood by the lay person, whereas the much more subtle and yet in no way less real genetical dangers are not easily appreciated. It is the great virtue of Dr. Weaver's lecture that it states quite clearly and unequivocally those basic facts of radiation-induced mutation about which there is general agreement. He summarizes them in a paragraph which seems so important to me that I should like to quote it in full: "First of all, the change produced by mutation is practically always a change for the worse. Second, the amount of mutation varies directly with the amount of radiation. Third, there is no minimum amount of radiation which is genetically safe—all radiation is genetically bad. A little radiation is a little bad, and a lot is a lot bad. Fourth, once exposed to some radiation, this never 'wears off'; this is to say, the genetically important number of mutations depends on the total dose that one accumulates from his own conception up to the time of conception of his last child. Fifth, the radiation that is important genetically is only that which reaches the gonads—that is to say, the male testicles and the female ovaries. Sixth, what counts from the point of view of society as a whole is the total number of mutated genes. Thus a small radiation dose to a large number of persons is, sociogenetically speaking, equivalent to a large dose to a few".

Finally, there is an excellent brief discussion of the reason why, in the face of this generally accepted situation, scientists yet differ among themselves in

* "Radiations and the Genetic Threat." By Warren Weaver. *Franklin Inst.*, **263**, No. 4 (April 1957).

their assessment of radiation hazards. This is only in part due to our lack of knowledge concerning the number of mutations which a given dose of radiation produces in man; in fact, the estimates of this quantity vary surprisingly little between scientists in Britain and in the United States. The main causes of divergent opinions are the personal attitudes to two problems: one temporal and the other numerical. The temporal: how large is our responsibility towards future, still unborn, generations of mankind? The numerical: granted that a certain dose of radiation is sure to cause the death of a few thousand babies in the next generation, do we need to worry about this when these deaths, for which we carry the responsibility, form only a very small proportion of 'natural' deaths? The answers to these questions cannot be dictated by the scientist; they have to be given by everyone according to his reason and moral standards. All the scientist can do is to put the facts before the public, and this has been done most ably in Dr. Weaver's lecture.

C. Auerbach

[1] Auerbach, C., *Nature*, **178**, 453 (1956).

INSTITUTE OF PHYSICS

REPORT FOR 1956

The thirty-seventh annual report of the Board of the Institute of Physics covering the work of the Institute during 1956 was presented to the annual general meeting of the Institute, which was held on July 12 at Oxford during the Institute's third convention. During the year under review the Board met six times and its various standing committees ten times. The membership and examinations committee dealt with 752 applications for election or transfer to the various grades of membership, and the interviews panel met three times and interviewed eleven candidates. Representatives of the committee visited four technical colleges which had applied for recognition by the Institute. The committee is now also responsible for giving advice on educational matters, and two panels have been set up—one to consider the revision of the lists of recognized qualifications for membership, and the other to submit a report to the Board on suggested means of stimulating entry to the profession of physics.

The total membership of the Institute rose during 1956 by 385 to 5,531, with the greatest increase in the associateship (203) and the graduateship (104) grades of membership. The graduateship examination was held during June 11–15, and written papers were taken both at the Institute's rooms in London and at three provincial centres. Seven of the forty-five candidates who presented themselves for examination satisfied the examiners. For the final examinations for National Certificates in Applied Physics the numbers of candidates were 424 for the ordinary level and 152 for the higher certificate, compared with 309 and 82, respectively, in 1955. The Board agreed to accept the diploma in physics of the Northern Polytechnic, London, as giving complete exemption from the Institute's academic requirements for graduateship membership. Five such diplomas had been accepted during 1955, three of which were to be awarded on satisfactory completion of sandwich courses. In co-operation with the London and Home Counties Regional Advisory Council for Higher Technological Education a conference on "Degree and Diploma Courses in Applied Physics" was arranged and held at the Institution of Electrical Engineers, London, during November 15–16. More than half the 160 who attended were senior representatives of industry or government service. The proceedings of the conference, including the discussion, have since been published, both in the Institute's *Bulletin* (8, 47, 86, 119, 166; 1957), and separately in booklet form. ("The Education of Physicists in Universities and Colleges of Technology": Institute of Physics, London. 1957. 6*s*.)

The increase in circulation of the Institute's two journals, *The Journal of Scientific Instruments* and *The British Journal of Applied Physics*, to libraries and non-members was about double that of the past few years, and the effect on the circulation of the new requirement that members pay for their own copies was less than anticipated. The revenue from advertisements showed a satisfactory increase. Details of the numbers of contributions submitted and published, together with the extents of the journals, are given in the annual report. The average delay between the first receipt and publication of a manuscript remained about six months. Supplement No. 5 of *The British Journal of Applied Physics*, containing the proceedings of the conference on "The Physics of Nuclear Reactors", held in London during July 3–6, was published in November.

The *Bulletin*, which has been expanded and presented in an entirely new and improved format during 1956, is now the only periodical of the Institute to which all members are entitled without extra payment, and its development is considered by the Board as an important part of the publication policy of the Institute. The twelve monthly issues comprised 344 pages containing twenty-three articles; news of the activities of the Institute's branches and groups; thirty-six book reviews; and the titles and abstracts of the principal contents of the Institute's two journals.

The Institute continues to be represented on many joint bodies and committees, including the Parliamentary and Scientific Committee, committees of the Royal Society and the British Standards Institution, and several national, regional and other scientific and technological advisory councils. Extracts from the reports of the Institute's representatives are given in the annual report. The number and nature of the inquiries addressed to the Institute concerning professional matters were much the same as in previous years. A fourth in the annual series of surveys of salaries and emoluments of members of the Institute was made in October and a report on the survey was published in the January issue of the *Bulletin* (8, 19; 1957). The annual inquiry concerning the type of posts taken up by new graduates in physics was extended in the case of 1955 graduates to include those with M.Sc. and Ph.D. degrees. A similar inquiry is being made in respect of 1956 and 1957 graduates, and it is hoped to publish a report, with comments, which will cover the results of these inquiries.

Details of the activities of the specialist groups and of the branches of the Institute, both home and overseas, are listed in the report. The Australian branch consists of 419 members and separate divisions in the various states of the Commonwealth. Under the auspices of the New South Wales division a conference on contemporary optics was held in Sydney

during September 19–21 (*J. Sci. Instr.*, 34, 129; 1957). The inaugural meeting of the Malayan branch was held in Singapore on May 9 when Mr. C. G. Webb was elected chairman and Dr. Thong Saw Pak honorary secretary. The Liverpool and North Wales branch held a two-day symposium in Bangor during September 26–27 on the subject of nuclear magnetic resonance, at which it was decided to form a Radiofrequency Spectroscopy Group of the Institute to cover all branches of spectroscopy up to about 10^{11} c./s. The programme of the London and Home Counties branch included a joint symposium with the Royal Institute of Chemistry and the Institute of Biology on the presentation of science to the public, and visits to the National Institute for Medical Research, the Research Laboratory of Associated Electrical Industries, Ltd., at Aldermaston, and the Royal Naval College and the National Maritime Museum at Greenwich. A one-day conference on physics in industry was held in Cardiff by the South Wales branch at which Sir John Cockcroft gave a public address on "The Future of Science and Technology in Industry" (*Bulletin*, 7, 147; 1956). Joint meetings were held between the Education Group and the Manchester and District branch, and between the X-ray Analysis Group and the Low Temperature Group of the Physical Society; a symposium on thin films was held by the Electronics Group in Reading; and the summer meeting of the Non-Destructive Testing Group took the form of a symposium in Bristol on the physics of some new aspects of non-destructive testing, the contributions to which have since been published (*Brit. J. App. Phys.*, 8, Suppl. No. 6; 1957).

At the general meeting of the Institute, the following were elected to take office on October 1: *President*, Mr. O. W. Humphreys; *Vice-President*, Prof. E. C. Stoner; *Honorary Treasurer*, Dr. J. Taylor; *Honorary Secretary*, Prof. F. A. Vick.

S. WEINTROUB

WALL DEPOSITION IN *CHAETOMORPHA MELAGONIUM* (CLADOPHORALES)

By DR. E. NICOLAI

Botany Department, University of Leeds

THE morphological complexity of the structure of the walls in several members of the Cladophorales attracted the attention of botanists as early as the middle of the past century, at a time when the light microscope was the only tool available. Since that time, the application of the more modern methods of X-ray diffraction analysis and electron microscopy has confirmed the older results and amplified them by adding structural details formerly beyond the limits of investigation. It is now known[1] that the walls of all the members of the Cladophorales—except the genera *Acrosiphonia* and *Spongomorpha*—are built up of many lamellæ each consisting of strictly parallel microfibrils imbedded in an amorphous matrix. The direction of these microfibrils changes from layer to layer through an angle of about 90°, so that by and large in every other layer the microfibrils are running in the same direction[2]. This structure, the 'cross-striation pattern', had been foreshadowed in the light-microscope observations of two sets of fine striations visible in surface view of the walls, reflecting exactly the directions of the two sets of underlying microfibrils[3]. Approximately the same structure occurs in the walls of *Valonia* where, however, a third direction of orientation is present, though the corresponding microfibrils are less abundant than are those in the two major directions[4]. In the filamentous Cladophoraceae, one of the sets of striations (and therefore one set of microfibrils) is approximately parallel and the other one perpendicular to the longitudinal axis of the cells.

X-ray analysis has shown that the microfibrils in the above-mentioned cases consist of highly crystalline cellulose I[2] and in *Valonia* this has been confirmed by electron diffraction analysis[5].

It is clear that any ideas on the genesis of this crossed fibrillar structure are best based on observations of walls recently formed at a previously naked cytoplasmic surface. The swarmers and the developing sporelings of members of the Cladophorales obviously offer suitable material on which critical observations might be made, and the present article concerns the data obtained about the first wall laid down and the stage of development at which the cross-striation pattern makes its appearance.

Two marine members of the Cladophorales were chosen because of the ease and reliability with which they form motile stages in quantity. One of them will be discussed here, *Chaetomorpha melagonium*. The unbranched filaments of this plant were collected on the Northumbrian coast, near the Dove Marine Laboratory in Cullercoats. The motile stages of reproduction can be either gametes, the fusion of which gives a zygote which in turn develops into a new plant, or zoospores which sprout without fusion. During the months of January and February, filaments brought from the coast, kept overnight in a refrigerator at about 2° C. and then returned to room temperature in the light, produced large numbers of motile cells. Many of the large cells of the filaments were emptied in the formation of about 8μ-long swarmers, each bearing two flagella. The presence of two flagella and the emptying of whole filaments both in the laboratory and on the shore suggests, on the basis of Hartmann's work[6], that these are gametes, though only very rarely could a beginning of fusion be observed. These swarmers, nevertheless, can be induced to settle and germinate. They keep moving for several hours and collect towards the light. Though they settle readily on solid substrates, it proved to be difficult to find a transparent medium on which they sprout and develop. The best results were reached with silica plates covered with sea water. On this substrate settling and sprouting of

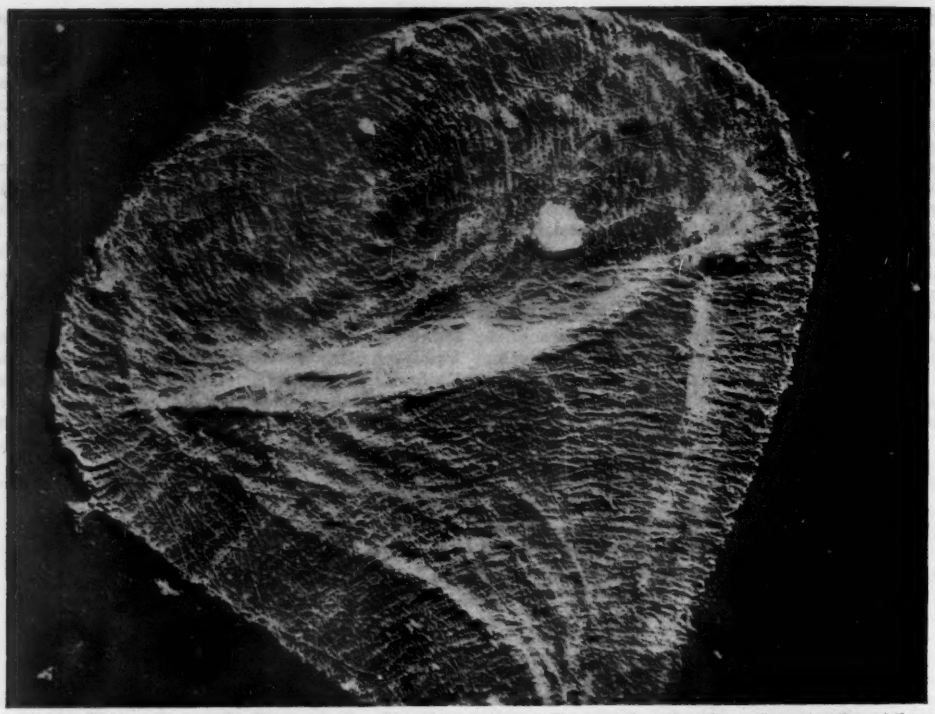

Fig. 1. Electron micrograph of the wall of a 24-hr. old sporeling of *Chaetomorpha melagonium*, after extraction of the contents. (× *c.* 11,800.) Shadowed with palladium-gold. The sprouting end is below the bottom of the photograph

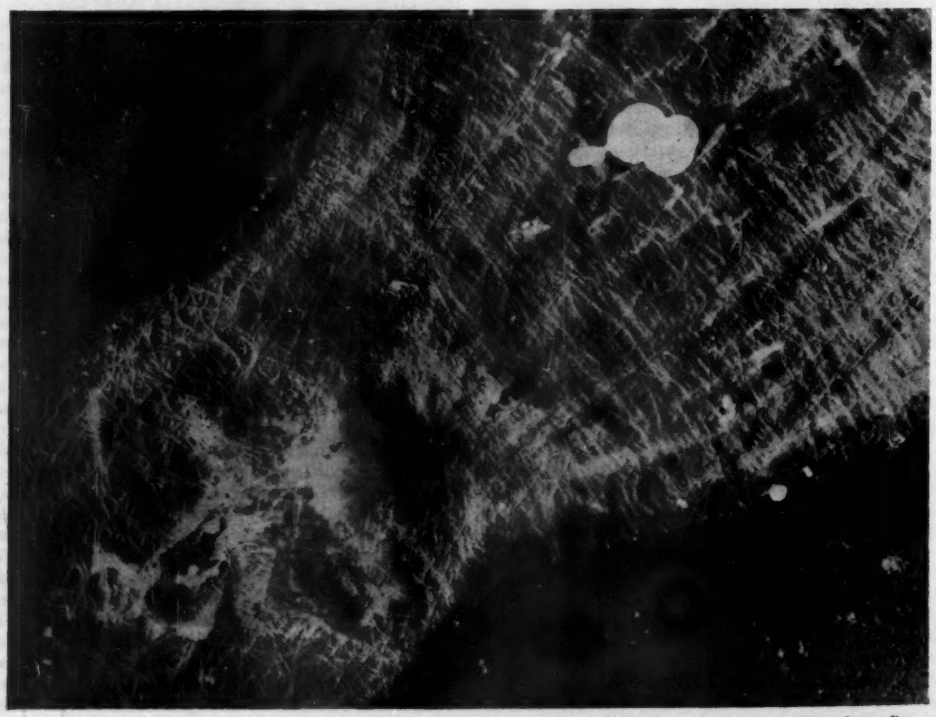

Fig. 2. Electron micrograph of the wall at the sprouting end of an 11-day-old sporeling of *Chaetomorpha melagonium*, after extraction of the contents. (×*c.* 17,500.) Shadowed with palladium-gold

the swarmers can be observed under the microscope, and the sporelings can be easily brushed off and collected at any stage required for investigation. The cultures were kept at a temperature of ± 5° C. in a special refrigerating unit provided with strip-lights and designed for the growing of algae.

Investigation of the structure of the different stages of development was carried out by means of X-ray diffraction analysis and by electron microscopy. For both methods large quantities of swarmers or sporelings had to be available.

X-ray diagrams were obtained by packing swarmers or sporelings into a hole about 1 mm. diameter in a 'Perspex' holder which was mounted over the slit of the X-ray spectrometer. Electron micrographs could be obtained only after pretreatment of the material, as the contents of the cells make them opaque to the electron beam. The extraction method used by Thimann and Bonner[7] in their investigation of coleoptiles proved to give good results in this case. The sporelings were washed in distilled water, boiled for about ½ hr. in water, kept for 24 hr. in a ½ per cent solution of ammonium oxalate at a temperature of 80° C. and then extracted successively in boiling 2 per cent sulphuric acid and in boiling 2 per cent sodium hydroxide. After thorough washing in distilled water, the material was caught on a specimen holder covered with 'Formvar' film and shadowed with palladium–gold. Electron micrographs of different stages of development of swarmers have been made.

(*a*) *Swarmers before settling.* Electron micrographs of swarmers caught on the 'Formvar' film while still swimming, dried and shadowed, present no features of importance since the contents are opaque to the electron beam. It is clear, however, that the cells have burst on drying, and that there is no trace of a wall or a boundary layer.

(*b*) *Swarmers after settling.* It can readily be observed in the light microscope that a wall is formed shortly after settling.

Electron micrographs of this material were taken within 24 hr. of settling. After extraction, the sporelings yielded clear pictures of an already well-developed wall (Fig. 1). The most striking observation at this stage is the presence of a well-defined pole opposite the region at which sprouting occurs, the beginning of which can also be observed already in this early stage. As the swarmers settle with their apical part (where the flagella were attached) to the substrate, and sprout, the pole must have developed at the posterior part of the original cell. Around the pole, coarse bands may be seen encircling the cell. On the outer side of these bands distinct and very thin microfibrils are present which are, however, isolated and infrequent. They tend to radiate from the pole and to converge towards the sprouting end, where after two days a random structure of well-defined fibrils is present. There is a suggestion of the presence of microfibrils in the coarse band structure below.

The next stage which proves to be of interest is that of the 11-day-old sporelings, which show after extraction a wall with a cross-striation pattern (Fig. 2). The more-or-less random microfibrils at the outside are still visible, and the original coarse bands around the cell now show microfibrillar structure, whereas towards the inside of the cell a second coarse pattern of broad, more or less opaque bands has developed, perpendicular to the first one. The fibrils in this pattern are still chiefly hidden in the opaque bands and are visible only occasionally. The sprouting part of the sporeling has increased in length and still has a random fibrillar orientation.

In comparing the X-ray diagrams of these stages of development note must be taken of the fact that only in the 11-day-old sporelings is a clear diagram of highly crystalline cellulose I obtained. In the younger stages vague rings are present corresponding to some of the spacings in cellulose I, notably the 3·9 A. and the 5·4 A. spacing. The ring corresponding to a spacing of 4·4 A. is by far the most prominent. Apart from these spacings, a few additional ones are present not belonging to cellulose I. Increase in age of the sporelings leads to a gradual decrease in the intensity of the ring corresponding to a 4·4 A. spacing, together with an increase in the intensity at 3·9 A. In the 11-day-old sporelings, the complete cellulose I diagram is present with all spacings in the correct relative intensity, whereas the additional spacings have disappeared. An electron diffraction pattern of a sporeling in this stage shows two crossed systems of orientated cellulose spacings.

One of the major points of interest in the development from motile stages to sporelings in *Chaetomorpha melagonium* is thus the presence of a pole in the very first stages of wall-deposition as the centre of a well-defined structure. In adult vesicles of *Valonia* the presence of a pole has been firmly established[4] only after many years of extensive research; in *Chaetomorpha* sporelings, however, electron micrographs show its presence without any shadow of a doubt. The structure of the encircling coarse and slightly opaque bands does not reveal the presence of well-defined microfibrils in the first instance.

After some time (in this case, 11 days), however, microfibrils are visible within these bands, though in several places they still seem to be cemented together by an opaque substance. The impression is gained that a crystallization of cellulose molecules to microfibrils with a cellulose I lattice takes place gradually within the bands. Whether some protoplasmic component is still, at first, intimately linked with the cellulose molecules and gives the above-mentioned X-ray diagram is a matter for speculation. It is certain that in the 11-day-old wall where the transverse bands show fibrils and where a second coarse and still partly opaque band structure has developed, perpendicular to the first one, a cellulose I X-ray diagram is recorded for the first time in the growing sporelings, whereas an electron diffraction pattern obtained from one cell shows the presence of two well-orientated systems perpendicular to each other.

During this development the outer part of the wall still shows the random fibrils which probably are not built of cellulose I, as the X-ray diagrams in the first stages did not show the cellulose I spacings.

I wish to acknowledge with gratitude the valuable contribution of Mr. G. W. Ripley, who took the electron micrographs, and my sincere thanks are due to Prof. R. D. Preston for his continued help and advice.

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LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

A Suggested Statistical Model of some Time Series which occur in Nature

The following is an amplification and extension of some work which was described in a general way at a symposium of the Research Section of the Royal Statistical Society on Storage Problems on March 6, 1957.

Annual values of some phenomena such as river discharges, rainfall, temperature, are approximately normally distributed if no account is taken of order of occurrence. Their principal characteristic, however, is the occurrence of periods when, on the whole, values tend to be high and others when they tend to be low, though low values may occur in a high period and vice versa. So far as is known, there is no regularity in the occurrence or the length of these periods, and usually there is no significant correlation over one of them between a year and its successor.

This phenomenon is important in problems of storage. In these a quantity R enters, which is obtained by taking departures of the annual values from their mean for a period of N years, and plotting the continued sums of these departures against the numbers 0 to N. The range of this curve is R and is easily seen to be, in the case of a river, the storage capacity which would have been necessary to enable the mean discharge to flow in every year of the period. In the case of natural phenomena, of which about a hundred have been examined[1], the following equation represents the mean result:

$$R/\sigma = (N/2)^K \qquad (1)$$

where σ is the standard deviation and K is a quantity which has a normal distribution and the mean of which is 0·73.

If, however, the quantities considered are entirely independent events, such, for example, as would arise from tossing a set of, say, twelve coins and recording the differences between the number of heads and number of tails at each throw, R is represented by the following equation:

$$R/\sigma = \sqrt{\tfrac{1}{2}N\pi} = 1{\cdot}25\sqrt{N} \qquad (2)$$

The values of R are the same from the two equations when $N = 22$; but for higher values of N the value of R from equation 1 steadily increases at a greater rate than does R from equation 2. For $N = 100$ the difference is about 25 per cent, and for $N = 1{,}000$, 100 per cent. There is therefore a definite difference between many natural time series and those where the terms are independent of each other.

An apparatus by which either type of series can be imitated is a pack of what I have called 'probability cards'. The first pack contained fifty-two cards numbered ± 1, ± 3, ± 5, ± 7, and the numbers of each kind of card were thirteen of the ones, eight of the threes, four of the fives, and one of the sevens. These numbers are proportional to the ordinates of the normal curve corresponding to departures of 1, 3, 5 and 7 from the mean 0. If cards are cut from this pack, shuffling after each cut, a series of random numbers will be produced which will conform to equation 2, and this has been found to be the case[1].

If, however, the following procedure is adopted, a different result will be obtained. The pack is shuffled and a card is cut, and after its number is noted it is replaced in the pack. Two hands are then dealt, and if, for example, the card cut was + 3, the highest three positive cards in one hand are transferred to the other, and the highest three negative cards are removed from it. This hand then has a definite positive bias. A joker is now placed in it and it is shuffled and a card cut from it. The number on this card is the first of the series. It is replaced and the hand then reshuffled and another card is cut and recorded. This cutting and shuffling goes on until the joker is cut. Then the joker is removed and all the cards are put together and shuffled, after which the process is repeated. The amount of bias, the number of cuts for which the bias acts, and the actual cards which turn up all depend on random processes.

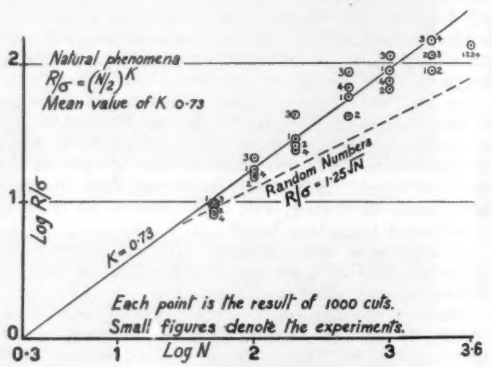

Fig. 1. Cutting probability cards. Pack of fifty-two. Biased hand of twenty-six cards + joker

Four different experiments of 1,000 cuts each were carried out. From each of these R was determined for twenty sections of fifty cuts, ten sections of a hundred, nine overlapping sections of two hundred, etc. The points up to $N = 1{,}000$ are the means of these sets. The four experiments were combined in order of occurrence so as to give values for 2,000 and 4,000 cuts. The results are shown in Fig. 1, where small figures alongside the observed points indicate to which experiment they belong. The mean value of K from all experiments is 0·71.

The card experiments have not been fully analysed as to variation of means and standard deviations, and further experiments are also being made. The result, however, indicates that the process described leads to time series very similar to those which occur in Nature.

H. E. Hurst

Sandford on Thames,
Oxford.
May 11.

[1] Hurst, H. E., *Trans. Amer. Soc. Civ. Eng.*, **116**, 770 (1951); *Proc. Inst. Civ. Eng.*, Pt. 1, **5**, 519 (1956).

Velocity of the Seismic Waves *Lg* and *Rg* across Australia

An examination has been made of *Lg* and *Rg* phases recorded over continental paths in Australia. The results which are here presented show some striking agreements with the earlier work of Ewing and Press[1] for North America and of Båth[2] for Eurasia.

The "International Seismological Summary" lists seven Australian earthquakes well recorded overseas, namely, the two Western Australian ones of August 16, 1929, and April 29, 1941, and the five Central Australian ones of December 20, 1937, April 17, 1938, March 26, 1939, May 4, 1941, and June 27, 1941. The epicentres and origin times of these earthquakes were revised using the method of Jeffreys and Bullen[3]; (the shock of March 26, 1939, has previously been discussed[4]).

The Riverview Observatory records of these seven earthquakes and of the Adelaide earthquake[5] of March 1, 1954, were those examined; the records were from Weichert instruments in the case of the first seven, and Galitzin instruments in the last case. The *Lg* phase was found to be recorded in the case of all earthquakes except that of August 16, 1929, a marginal earthquake off the north-west coast. The *Rg* phase was clear only for those of May 4, 1941, and March 1, 1954. (The *Lg* and *Rg* phases were identified in all instances without prior calculation of their expected arrival times.)

The mean *Lg* group velocity was $3{\cdot}50 \pm 0{\cdot}08$ km./sec. for seven observations from the vertical component instruments, and $3{\cdot}50 \pm 0{\cdot}06$ km./sec. for thirteen observations from the horizontal component instruments. In most cases the *Lg* phase was predominant on the records and its start had been given as an impetus onset in the *Riverview Seismological Bulletin*. Its initial period was 3–6 sec. The mean *Rg* group velocity was $3{\cdot}03 \pm 0{\cdot}07$ for three observations.

Press and Ewing, using about forty earthquakes, have given the mean group velocities of *Lg* and *Rg* over North American paths as $3{\cdot}51 \pm 0{\cdot}07$ and $3{\cdot}05 \pm 0{\cdot}07$ km./sec. respectively. For Eurasia, using about four hundred earthquakes, Båth has found at least two distinct *Lg* group velocities, namely, $3{\cdot}54 \pm 0{\cdot}07$ km./sec. and $3{\cdot}37 \pm 0{\cdot}04$ km./sec. Båth also found a mean *Rg* group velocity of $3{\cdot}07 \pm 0{\cdot}10$ km./sec.

The Australian data do not indicate strongly any separation of the *Lg* phase; only the earthquake of June 27, 1941, showed an *Lg* velocity, namely, 3·38 km./sec., which differed from the mean by more than the standard error. The combined mean Australian *Lg* velocity is $3{\cdot}50 \pm 0{\cdot}07$ km./sec., in striking agreement with the North American and Eurasian results. The importance of the present results is that they indicate that *Lg* and *Rg* velocities of order 3·50 and 3·05 km./sec. respectively are fairly world wide.

I am indebted to Fr. T. N. Burke-Gaffney, director of the Riverview Observatory, Sydney, for kindly making available the relevant records.

B. A. Bolt

Department of Applied Mathematics,
University of Sydney. May 9.

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[3] *Bur. Centr. Seis. Trav. Sci.*, No. 11 (1935).
[4] Bullen and Bolt, *J. Roy. Soc. N.S.W.*, **90**, 19 (1956).
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An Attempt to detect Linearly Polarized Radio Emmision from the Galaxy

Several authors[1,2] have suggested that synchrotron radiation by relativistic electrons in galactic magnetic fields may be responsible for part of the galactic background radiation at radio-frequencies. Radiation so produced is linearly polarized in a direction perpendicular to the magnetic field. The actual polarization observed from an extended region is likely to be reduced due to a number of mechanisms[3,4] which will be discussed elsewhere, but a small percentage might remain. Razin[5] has described an attempt to measure this polarization by a method based on the suggestion that the Faraday effect in interstellar space would make the angular position of the plane of polarization strongly dependent upon frequency. By switching between a wide and a narrow receiver band-width, a change of output was noted which was interpreted as implying the presence of up to 4 per cent of linear polarization in some directions. On present evidence, the magnitude of the galactic magnetic field and of the electron density in interstellar space[6] are thought insufficient for this dependence on frequency to lead to a reliable method of measurement, and a different procedure has been adopted. In the present method, a receiver band-width of 4 Mc./s. has been used, and although in certain circumstances the wider band-width might lead to a reduction of the polarization observed, the sensitivity is much improved, particularly when observing in directions away from the galactic plane.

A 7·5-m. paraboloidal reflector mounted as a transit instrument was used, at a frequency of 159·5 Mc./s.; at the focus, two dipole and reflector systems were mounted with their planes at right angles. This assembly was motor-driven about the axis of symmetry of the aerial system at a speed of one revolution in 27 min. The dipoles were connected to a phase-switching receiver[6] which gives an output proportional to the time average of the product of their voltages. For linearly polarized radiation incident along the axis of the system, correlated voltages appear in the dipoles and an output is produced; randomly polarized radiation along the axis produces uncorrelated voltages and gives no output. As the dipoles rotate, a sinusoidal record is produced, and from the amplitude and phase of this record the magnitude and position angle of any linearly polarized component may be determined. For this type of aerial system, intense randomly polarized radiation from directions off the axis of the aerial does, however, give a small response. Hence when the system is used to examine the galactic background radiation, a spurious apparent polarization may be produced. The magnitude of this effect has been estimated using the sun as source, and the observed effects agree with those estimated.

In order to overcome this difficulty, use has been made of the rotation of the plane of polarization by the Faraday effect in the terrestrial ionosphere, and the initial observations have been made at a declination of 22° N. where the line of sight is parallel to the geomagnetic field. Observations were made throughout the month of February 1957, and the progressive displacement of the solar time of observation of any particular point in the sky results in the observations being made through a progressively varying amount of ionization. This change of total ionization along

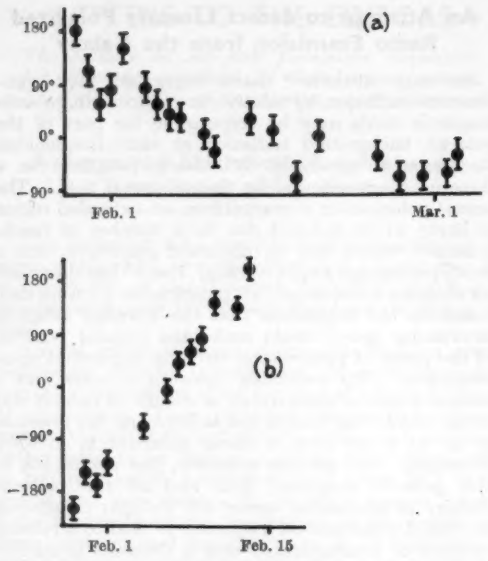

Fig. 1. Plots of the position angle against time of the planes of polarization for the areas centred on (*a*) R.A. 05h., declination 22° N.; (*b*) R.A. 18h., declination 22° N. The origin of the ordinate corresponds to polarization in the plane of the meridian

the line of sight causes a progressive rotation of the plane of polarization of any polarized component that may be present, while the spurious deflexion is unaffected. Diurnal curves of total electron content up to the maximum of electron density (Thomas, J. O., private communication) showed that during the month about one complete rotation of the plane of polarization would be expected, both in the evening when the electron content is declining, and in the early morning when it is increasing. Observations made during the former time should be more reliable, because the sun is then below the horizon; morning observations with the sun in the sky are complicated by a further spurious effect due to the direct reception of solar radiation by the primary dipoles. A vector construction was used for separating the rotating component due to actual polarization from the spurious effect.

The area in transit in the evening was from 04h. to 08h. R.A., a strip crossing the galactic plane approximately at the anticentre, where the brightness temperature is about 400 deg. K. An upper limit of 1 per cent can be put to the polarization throughout most of this region. It is believed, however, that linear polarization of the order of 1 per cent may have been detected at 05h., a region about 10° south of the galactic plane close to the anticentre. The area in transit in the morning was that around 17h.–18h., on the bright large-scale feature of the galactic emission described by Baldwin[7] and others, which rises from the galactic plane at about $l = 15°$. Here the brightness temperature is about 650 deg. K., and again it is believed that polarization of the order of 1 per cent has been detected. Fig. 1(*a*) and Fig. 1(*b*) show plots against time of the position angle of the plane of polarization for the regions near 05h. and 18h. respectively. The variation of the position angle in each case is in the direction to be expected from ionospheric data (Thomas, J. O., private communication), and is of the expected order of magnitude.

This work was carried out as part of a programme of radio astronomical research at the Cavendish Laboratory, supported by the Department of Scientific and Industrial Research, during the tenure of an Isaac Newton Studentship. I am indebted to Mr. M. Ryle and to Dr. F. Graham Smith for their help and advice at every stage of the work.

J. M. Thomson

Cavendish Laboratory,
Cambridge.
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Arrays with Constant Beam-Width over a Wide Frequency-Range

In many sound-ranging and listening systems (principally used under water) it is necessary to receive signals over a wide range of frequency, the ratio of upper to lower frequency being of the order of ten or more. Transducers (or hydrophones) with a sufficiently wide frequency response are available, but usually have to be used as omni-directional receivers because otherwise their beam-width would be inversely proportional to frequency. Clearly, the use of directional receivers would be very advantageous in many applications, but an essential condition is that the beam-width must be constant over the appropriate range of frequencies. An approach

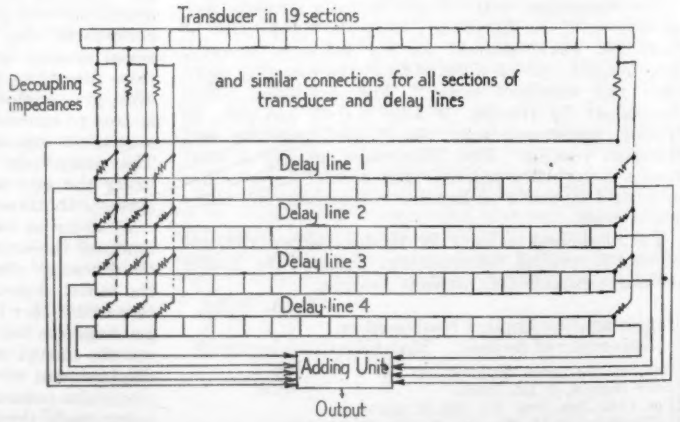

Fig. 1. General scheme of constant beam-width array

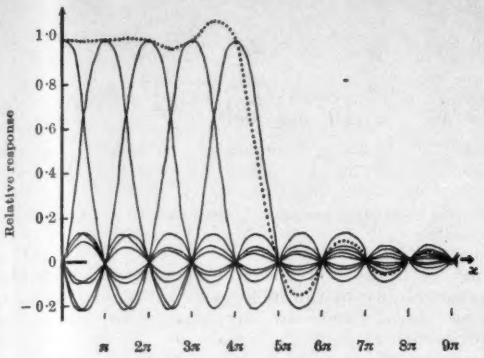

Fig. 2. Right-hand half of beam patterns at upper frequency limit for $r = 4$; $a_m = 1$ for all values of m. Component patterns, ———; resultant pattern, ... It is assumed that the number of sections in the array is large

to the solution of this problem is to connect the outermost units of the transducer to the receiving amplifiers through low-pass filters, so that the transducer is virtually made smaller at the higher frequencies. There are, however, many difficulties and complications in this process, which is, at best, rather crude.

A more elegant solution of the problem, and one in which the design problems are more straightforward, can be applied in the case of strip (or line) transducers, which are more suitably referred to as arrays of individual small transducers arranged in a straight line. Directionality is achieved in all planes containing the line of the array. The solution is based on the synthesis[1-3] of directional patterns from elementary $(\sin x)/x$ patterns using delay lines to produce these patterns at various angles of deflexion from the normal. The basic idea is that when the beam-width between half-power points is to be constant over a frequency range of ratio $2r + 1$, then $2r + 1$ patterns are superposed. One of these is the straight undeflected pattern of the array and the remainder is the set of patterns obtained from the $2r$ outputs of r delay lines. There must be a minimum of $2r + 1$ sections (or elements) in the array. The way in which these are connected is shown in Fig. 1 for $r = 4$ and for a 19-section array. Beam deflexion angles will be quoted in terms of x, where $x = \frac{l\pi}{\lambda} \sin \theta$,

l being the length of the array, λ the wave-length, and θ the angle of any physical direction with respect to the normal to the line of the array. Each delay line has a phase-shift which varies linearly from zero at the lower frequency limit to a value at the higher frequency limit which causes a beam deflexion of π rad. on the first delay line, 2π rad. on the second, and so on until the final delay line, where it is $r\pi$ rad. It is shown in ref. 1 that to give a deflexion on the x-scale of π rad., the total phase-shift in the delay line has to be 2π rad., assuming the number of transducer sections in the array is large.

The patterns need not all have the same amplitude, although symmetry should be preserved. If the outputs of the mth delay line have a relative amplitude a_m, then it is clear that at the lowest frequency, where the phase-shifts are zero, the overall pattern is merely

$$\left[\sum_{m=-r}^{m=+r} a_m\right] \cdot \frac{\sin x}{x}$$

As the frequency increases, so the patterns derived from the delay lines are spread out from the axis of the beam, thus widening the beam on the x-scale at the same rate at which the x-scale itself increases relative to the physical angles (θ) due to the decreasing wave-length (λ). A constant beam-width thus tends to be maintained. (An increase of frequency beyond the design maximum will separate the patterns so that dips occur in between them.) If we take the minimum frequency to have unity angular velocity, so that the highest frequency has the angular velocity of $2r + 1$, then at any frequency the directional pattern is

$$D(x) = \sum_{m=-r}^{m=+r} a_m \cdot \frac{\sin [x - m(\omega - 1)\pi/2r]}{[x - m(\omega - 1)\pi/2r]}$$

If $a_m = 1$ for all values of m, then a very flat-topped beam is obtained at the upper frequency, as illustrated in Fig. 2; and an almost exactly constant beam-width (measured between the half-power points) and a fairly constant directivity index are maintained. To maintain a reasonably constant shape of beam—as well as a fairly constant width—over the frequency range, a_m should diminish as m increases; but if the shape is to be maintained outside the half-power beam-width, then additional delay lines will be needed to give additional component patterns of greater angular deflexion.

The method may well also have applications in radio aerials.

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Adiabatic Piezo-Optic Coefficient of Heavy Water

Among the physical properties of heavy water, its refractive index, which has been studied widely, has been utilized quite often for the estimation of heavy water content, although other methods, such as infra-red absorption, are also used. But the change of refractive index of heavy water with pressure was studied recently by us, and in view of its significance for the formulation of any theory for the optical properties of liquids we present here a brief account of the constants for ordinary water and heavy water.

The adiabatic piezo-optic coefficient was determined by an interferometric technique[1] employing a Rayleigh–Zeiss refractometer, and adequate precautions were taken to eliminate errors due to expansion of the cell, etc. The measured value of the adiabatic piezo-optic coefficient for sodium D line (5893 A.) was $10{\cdot}51 \times 10^{-6}$ per atmosphere for heavy water (purity 99·8 per cent) as compared to $14{\cdot}66 \times 10^{-6}$ for ordinary water[1]. Since the expansion coefficient for this concentration of heavy water was not available in the literature, a dilatometric measurement of the same was also made. These values along with other relevant constants are

Table 1

	Temp. (° C.)	n	ρ	$\alpha \times 10^4$	c_p	$\beta_t \times 10^6$	$\beta_\varphi \times 10^6$
1. Water	23·1	1·3326	0·9976	23·84	1·000	46·06	45·6
2. Heavy water (99·8 per cent)	22·0	1·3283	1·1042	22·82	1·0062	48·5	47·0
	$\left(\frac{\partial n}{\partial t}\right)_p \times 10^4$	$\left(\frac{\partial n}{\partial p}\right)_\varphi \times 10^6$	$-\frac{1}{\alpha}\left(\frac{\partial n}{\partial t}\right)_p$	$\frac{1}{\beta_t}\left(\frac{\partial n}{\partial p}\right)_t$	$\frac{1}{\beta_\varphi}\left(\frac{\partial n}{\partial p}\right)_\varphi$	$\frac{(n^2-1)(n^2+2)}{6n}$	$\left(\frac{\partial n}{\partial t}\right)_p \times 10^4$ calc.
1. Water	− 9·65	14·66	0·4048	0·3253	0·3215	0·3664	− 1·97
2. Heavy water (99·8 per cent)	− 7·99 − 7·6*	10·51	0·3501	0·2248	0·2236	0·3610	− 2·8

n, Refractive index; ρ, density; α, cubical expansion; C_p, specific heat at constant pressure; β, compressibility; t, φ, temperature entropy.
* Luten.

shown in Table 1 along with the corresponding values for ordinary water. The temperature variation of the refractive index of heavy water was measured by the usual method with the aid of a Pulfrich refractometer.

As has been pointed out earlier, the Lorenz–Lorentz refraction formula presupposes the refractive index to be a function of the density alone, and accordingly

$$-1/\alpha\left(\frac{\partial n}{\partial t}\right)_p = \frac{1}{\beta_\varphi}\left(\frac{\partial n}{\partial p}\right)_\varphi = \frac{1}{\beta_t}\left(\frac{\partial n}{\partial p}\right)_t = \frac{(n^2-1)(n^2+2)}{6n}$$

That the experimental facts are not in accordance with this has been shown by the measurements of Raman and Venkataraman[1] and the more recent investigations in this laboratory[2]. For water the difference between $\frac{1}{\beta_\varphi}\left(\frac{\partial n}{\partial p}\right)_\varphi$ and that predicted by the Lorenz–Lorentz relation is 0·0449, this difference for heavy water is nearly three times as much, namely, 0·1374. Further, detailed analysis shows that none of the other common refraction formulæ such as Newton–Drude, Gladstone–Dale and Eykman satisfactorily account for the observed piezo-optic behaviour of all the liquids. This has been attributed to the neglect of a pure temperature effect on the refractive index at constant density, as can be seen from the fact that the usual refraction formulæ do not account for the measured value of $-\frac{1}{\alpha}\left(\frac{\partial n}{\partial t}\right)_p$. The pure temperature effect for heavy water $(\partial n/\partial t)_p$ as calculated from the experimental value of the adiabatic piezo-optic coefficient turns out to be $-2{\cdot}8 \times 10^{-6}$, that is, 35 per cent of the value of $(\partial n/\partial t)_p$, while for ordinary water it is only about 20 per cent. Also, unlike in the case of water, for heavy water the value of $-\frac{1}{\alpha}\left(\frac{\partial n}{\partial t}\right)_p$ is less than that predicted by the Lorenz–Lorentz formula. From their measurements of the piezo-optic coefficients of liquids and the deviation from the Lorenz-Lorentz formula, Raman and Venkataraman pointed out the influence of association, polarity of the molecules and the anisotropy of the polarization field, and have emphasized the fact that the maximum deviation occurred in the case of carbon disulphide, a highly polar liquid. However, it is sufficient to mention that though the dielectric constants of ordinary water and heavy water are nearly the same, the deviations from the theory of refraction are far greater in heavy water. It leads us to conclude that while association may influence considerably the piezo-optic behaviour of liquids, it is the local anisotropy of the field around any molecule and the consequent changes in the polarizability of the molecules that should be taken into consideration in any theory for the optical properties of liquids[3].

A detailed paper on this subject describing the results obtained with other liquids also will be published in due course in the *Journal of the Indian Institute of Science*.

Our thanks are due to Dr. H. J. Bhabha, head of the Atomic Energy Establishment, India, for placing at our disposal the requisite quantity of heavy water.

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Study of Strain Distribution in Metals by the Determination of Local Electrode Potentials

When a piece of metal has been subjected to local plastic deformation, as, for example, by indentation, it is important in research work to be able to determine the distribution of strain and the elastic–plastic boundary. At least five methods are available for these determinations, and Williams and O'Neill[1] recently published results using explorations by low-load hardness tests. Samuels and Mulhearn[2] have pointed out the defects of some of the existing methods.

It occurred to one of us that determinations of local electrode potential of the deformed metal as used by Spring in 1903 might provide a further method of studying strain distribution. The method is known to have certain difficulties, but a prelim-

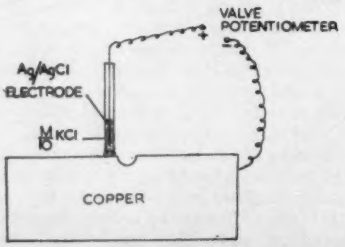

Fig. 1. Arrangement of electrode

inary test was made along the flat surface of a cylinder of annealed electrolytic copper which some years previously had been indented at its centre with a 10-mm. steel ball. The surface was cleaned with cotton wool and acetone, and the potential measurements were made with reference to a silver/silver chloride electrode as shown in Fig. 1.

The 'Pyrex' tube (internal diameter, 2 mm.) containing the electrode was immersed in $M/10$ potassium chloride and on withdrawing the electrolyte remained in it to a depth of 2 cm. The tube was then made to touch the copper surface momentarily, and on lifting it about 1 mm. from the surface, a liquid junction formed enabling potential measurements to be made. The potential of the specimen against that of the reference electrode rose with time but reached a reproducible steady value in ten minutes. This steady potential was found for various positions of the electrode on the copper surface. The potential in the impression itself and up to a distance of about 2½ cm. around it was anodic to the outside of the specimen, the difference between them being 10–12 mV. The values obtained for one filling of the tube in the sequence in which they were determined are given in Table 1.

Table 1

Distance from edge of impression in mm.	Potential versus Ag/AgCl in mV.
3	−190
7	−187
14	−187
Elastic/plastic boundary	
25	−180
37	−179
Inside impression	−190

Although the effect is small, it is reproducible, and should provide a method of exploring strain distribution. The test confirms the general results of Goerens[3] and of Simnad and Evans[4] for steel. Work is continuing on heterogeneous alloys.

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Relativity and Space Travel

May I suggest an alternative approach to this problem making use of the Fitzgerald–Lorentz contraction? Consider as before a pair of twins, one of whom (Traveller) makes the pilgrimage to Alpha Centauri, four light years away, and the other (Stayathome) does not. Before starting they set up identical crystal-controlled short-wave transmitters by means of which they can follow each other's progress and, with suitable frequency-dividers, operate their clocks.

Traveller now accelerates very quickly to, say, 99 per cent of the speed of light. Both twins will agree that their transmitters make only a small number of oscillations during the acceleration. (To decide whether their transmitters make the *same* small number of oscillations would involve the introduction of general relativity, which I do not propose. Whatever difference of clock-*times* this produces, can be swamped by differences due to change of clock *rates* if Traveller's journey is long enough.)

After the accelerations, each observes that the other's inertial frame is much contracted along the line of travel. For Stayathome this only means that his brother's spaceship looks some seven times shorter than it did. To Traveller it is now clear that the distance between Earth and Alpha Centauri is just under seven light-months—although the fact that Alpha Centauri's radiation is now mostly in the far ultra-violet makes recognition difficult. He sees, as expected, however, that Alpha Centauri is approaching him, and Earth leaving him, at 99 per cent of the speed of light. Consequently, the goal arrives and Traveller has to decelerate after seven months of Traveller's time, during which his transmitter has emitted seven-months-worth of oscillations. Having arrived at Alpha Centauri, therefore, Traveller is quite clear that he is only seven months older than when he started. He now re-accelerates to 0·99 c towards Earth. Again, the Fitzgerald–Lorentz contraction of the rest of the universe leads him to see Earth advancing towards him from a distance of only seven light-months; again his transmitter emits only seven months-worth of oscillations before Earth reaches him, and he decelerates to arrive just fourteen months older than when he started. Depending upon his degree of relativistic orthodoxy, he may or may not be disturbed to find Stayathome nearly seven years older than himself.

Whether or not they are disturbed, the brothers will not be surprised if they have been observing each other's signals regularly. During the outward journey the signals received by each brother from the other's transmitter have, of course, moved into the (same) long-wave region, at about 14 times lower frequency than the original.

While the frequencies observed are symmetrical, the times for which they are observed will differ. Stayathome observes his brother's ship travelling away from the Earth until he sees its arrival at Alpha Centauri. This arrival, in Stayathome's time, is a little more than four years from the start, but he does not observe it until the light has had time to return to Earth, that is, more than *eight* years from the start.

Traveller on the other hand, receives these long waves for seven months only of *his* time. As soon as he has started on the return journey, he notices a huge rise in the frequency of Stayathome's signals, to about 14 times its normal value. Traveller continues to observe these very-high-frequency signals for the whole seven months of Earth's return journey, as during this return he meets all of the waves which Stayathome has sent out in more than eight years. (Only the waves emitted by Stayathome during the first 0·04 year managed to catch up Traveller before he reached his goal.)

Stayathome, however, sees this same high frequency for a far shorter time. The turn-around signal from Alpha Centauri takes four years to arrive, and the ship itself takes only 0·04 year more. Consequently, he observes the high frequency for 0·04 year or 0·48 month only, against Traveller's whole seven months.

Thus, when they compare notes, Traveller has picked up a full 8·08 years worth of oscillations, while Stayathome has picked up only $(8{\cdot}04 \times 1/14) + (0{\cdot}04 \times 14)$ years—or nearly fourteen months-worth.

It only remains to mention a possible reason for the long continuance of the misunderstanding. Attention to the asymmetry of comfort due to the extremely 'high-g' take-off of Traveller, which all parties agree takes a negligible period in either time, has diverted attention from a more important asymmetry. This is that both goal and origin are in Stayathome's inertial frame, and we can record events at either of them in Stayathome's time without ambiguity. To give complete symmetry, we need to suppose a second space ship, presumably a Flying Saucer, coming from the side away from Alpha Centauri. It must be moving at the same velocity as Traveller and at such a distance that Traveller sees it to be four light-years behind him. Then to Stayathome, for whom the Traveller–Saucer frame has suffered a Fitzgerald–Lorentz contraction, Traveller and Saucer appear to be just under seven light-months apart. Accordingly, Stayathome observes Saucer's invasion of Earth seven months after Traveller's departure, when Traveller is only one-seventh of his way to Alpha Centauri. Traveller, however, considers that the Flying Saucer invasion occurs after four years of his (and Saucer's) time (by which time, having changed his mind about return, he has left Alpha Centauri far behind).

Thus we see that the conceptual problem underlying the controversy is the same as that involved in any of the standard paradoxes concerning the apparent simultaneity of separated events in different inertial frames.

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I have received several calculations similar to Dr. J. H. Fremlin's. To follow them through in detail is subtle and tedious, but it is unnecessary because it is at once obvious that since all the effects concerned are effects only of the relative motion of Stayathome and Traveller, and the motion of one is the mirror image of that of the other (for every stage of Traveller's motion, whether uniform or accelerated, there is an exactly corresponding stage of Stayathome's motion), there cannot possibly be any difference in the numbers of oscillations received.

Dr. Fremlin is right in ascribing many of the false conclusions which have been reached in this matter to the unconscious introduction of an asymmetry into what is essentially a symmetrical situation, but I do not think he has correctly identified that asymmetry. In saying that "both goal and origin are in Stayathome's inertial frame", he shows that he shares a common misunderstanding of the essential principle of relativity theory. Phenomena are not in any frame: we can place them in whatever frame we find convenient. What he means is that goal and origin (presumably goal means Alpha Centauri and origin the Earth) are relatively at rest: that is a fact which is quite independent of frames of reference.

Nevertheless, he comes near the essence of the asymmetry, which is that the halfway stage of the journey is, in the ordinary presentation of the problem, marked by an event (Traveller's arrival at Alpha Centauri) at which Traveller is present, and can, therefore, *observe* the time in his rest-system, while Stayathome is absent, and can only *calculate*, by the conventional definition, the time in his rest-system. Dr. Fremlin has himself shown how this artificial asymmetry can be reversed by marking the half-way stage by the arrival of the Earth at a Flying Saucer instead of the arrival of Traveller at Alpha Centauri. He spoils it, however, by saying that "Traveller . . . considers that the Flying Saucer invasion . . . (by which time, having changed his mind . . .)". Traveller considers the invasion to be an Earth invasion of Flying Saucer, and he does not change his mind at all: he sees the Earth immediately leave Flying Saucer and return to him. All Dr. Fremlin's previous reasoning then requires that the returning Stayathome will have aged seven months, and Traveller four years. I have shown this in a paper which will appear in the *Australian Journal of Physics* of September. To give the problem greater verisimilitude, instead of the Earth and stars I have considered an engine, M, travelling between two stations, A and B. The engine has a train attached, of rest-length equal to AB. When the turning-point is marked by M's arrival at B, M's clock is behind the time calculated according to the clock in A. When, however, it is marked by the arrival of the last truck, G, at A, the clock in A is behind the time calculated according to the clock in M.

If Dr. Fremlin will reconsider his statement that "both goal and origin are in Stayathome's inertial frame, and we can record events at either of them in Stayathome's time without ambiguity", I think he will see that he is unconsciously supposing that Stayathome's time is somehow superior to Traveller's. You can record events in *any* given time system without ambiguity. Ambiguity comes in when you have to choose between different systems, and you cannot resolve it by arbitrarily deciding for one of them.

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Rocket Observation of X-Ray Emission in a Solar Flare

The report[1] of normal Lyman alpha- and enhanced X-ray emissions from the Sun during a solar flare on July 20, 1956, presents several problems in connexion with observations of ionospheric absorption as well as the physics of the solar corona.

As is well known, during many flares the cosmic noise received on 18 Mc./s. drops sharply in intensity. The magnitude of the decrease can be used to derive information on the corresponding fractional increase in electron density through the absorbing layers of the ionosphere. On July 20, 1956, at 1905 U.T., our cosmic noise records showed a slight decrease in flux level, which fell at 1912 U.T. to its lowest value, about 93 per cent of the average over the preceding and following hours. The normal flux level was re-established by 1920 U.T. If we assume that the fractional increase in electron density was constant through the D-region, the maximum increase therefore amounted to about 7 per cent. On the other hand, the maximum increase during a typical cosmic noise absorption can be several hundred per cent. Chubb *et al.* state that the observed X-ray increase should have doubled the electron density between heights of 75 and 95 km. Such an increase is far greater than the increase consistent with our records. If we allow the possibility that the fractional change

in electron density varies with height, the function must increase sharply above the 75–80 km. level where the principal contribution to the absorption usually occurs. In this way it would be possible to have a large increase in electron density without corresponding increase in absorption. But if this were so, then the X-ray effects of the flare would modify the height distribution of electrons at the wrong heights for an explanation of the lowering of the *D*-region as determined by the method of phase anomalies.

On a purely Lyman alpha theory of the origin of the cosmic noise absorption, we would conclude that the total solar output was $(1{\cdot}07)^2$ times normal, a virtually undetectable (personal letter from H. Friedman) increase of only 14 per cent, even if the rocket had flown during the maximum phase of the cosmic noise absorption.

We now consider the relative merits of the Lyman alpha- and X-ray mechanisms for sudden ionospheric disturbance (S.I.D.) effects. Chubb *et al.* point out that some large flares do not show S.I.D. effects, and that this fact argues against the Lyman alpha mechanism for the production of the S.I.D. However, C. S. Warwick[2] has shown that the chromospheric opacity, combined with the varying heights of solar flares, provides a natural explanation for this phenomenon. This explanation cannot be used for X-ray effects, since the chromosphere is relatively transparent to X-radiation.

In order to explain the ionospheric effects, the Lyman alpha flux during a flare must increase by several orders of magnitude, corresponding to a flare excitation temperature between 2×10^4 and 3×10^4 deg. K. Observations[3] of flare kinetic temperatures yield values in excess of 3×10^4 deg. K. On the Lyman alpha hypothesis, during the largest flares, the total flare output is about 10^{30} ergs/sec., which is 10^{-3} solar constant. Although this may seem large, the cosmic-ray energy alone of the outstanding flare event of February 23, 1956, amounted[4] to some 10^{27} ergs/sec. In Lyman alpha we deal with the resonance line of the most abundant atom in Nature and therefore expect a much greater energy. Furthermore, flux of this order of magnitude explains[5] simply and directly the rectilinear motions and support of the outstanding prominences associated with flares.

There appear to be two simple mechanisms for the production of 3-A. X-rays in the solar corona: recombination and line emission. Elwert[6] shows that no significant flux is expected from a coronal condensation at 6×10^6 deg. K. on either mechanism. In the case of recombination, the recombining electron must have more than 3 keV. energy, which greatly exceeds the modal energy in the Maxwell–Boltzmann distribution at 6×10^6 deg. K. In the case of line emission, the existence of Fe XXV or Fe XXVI would be required, whereas in the normal corona iron is in the states Fe X–Fe XIV. Since the Sun is able to produce cosmic rays, one cannot rule out the possibility that it also emits 3-A. X-radiation, by some unknown mechanism. The existence of high-temperature regions, at 10^7 deg. K., would produce the required radiation. However, there is no optical evidence for such regions. Another possibility is that 3-A. X-rays are produced locally, in the Earth's atmosphere. The observation[7] of soft radiation in the auroral zone at low elevations supports such a conjecture.

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In reply to Warwick and Zirin's communication, we wish to comment first on their suggestion that the observed 3-A. X-rays were produced locally, in the Earth's atmosphere. The detector in our experiment is a photon counter mounted on the wall of the cylinder that encloses the instrumentation section of the rocket. The beryllium window of the counter lies almost flush with the surface of the cylinder and looks out in a direction normal to the long axis of the rocket. As the rocket rolls during flight, the photon counter can 'see' the Sun once per roll. During the flight, X-rays were detected only when the photon counter 'saw' the Sun. There can be no question but that the measured radiation came directly from the Sun and not from the Earth's atmosphere.

Any conclusion regarding the increase in *D*-region electron density resulting from the observed X-ray flux depends very much on the assumed values for recombination coefficients at different altitudes. Since submitting our original communication to *Nature*, we have made more detailed analyses of our observations. We estimate the total flux in the 3–8 A. band to have been 5×10^{-3} ergs/cm.^{2}/sec. From the dependence of intensity on altitude we found that the first decade of absorption took place roughly between 98 and 85 km.; the second decade between 85 and 78 km. Very little radiation penetrated below 78 km. Knowing the rate of absorption at any given level, an electron density can be computed if the recombination coefficient is known. Assuming the recombination coefficient α is 10^{-7} cm.^{3}/sec. between 85 and 98 km., we get 3×10^4 electrons/c.c. If α increases to 10^{-6} cm.^{3}/sec. between 78 and 85 km., we obtain about 4×10^3 electrons/c.c. in that range. According to estimates of the normal *D*-region based on radio measurements[1], a density of 3×10^4 per c.c. occurs at about 93 km., and 4×10^3 at 81 km. Hence the X-ray source should have increased the electron density in the *D*-layer by about 40 per cent.

As Warwick and Zirin suggest, the absorption effect is very sensitive to the altitude of the absorbing region (75–80 km.). The major effect of the X-ray ionization certainly occurred well above 80 km. It is not surprising, therefore, that the observed cosmic noise absorption was small and that no fade-out or other form of S.I.D. was observed. We would not expect to see an appreciable lowering of the base of the *D*-layer unless the flare were very much greater. However, because we observed an unusual X-ray increase in such a small flare, it seems plausible to

expect much greater X-ray emission accompanying a flare capable of producing an S.I.D.

We hope to obtain the experimental measurements needed to answer the question of the relative importance of Lyman α- and X-rays in S.I.D. phenomena from a series of rockets to be fired during the International Geophysical Year. Beginning July 1, fourteen Nike–Deacon rockets instrumented to measure Lyman α- and X-rays will be ready for firing from San Nicolas Is. in the Pacific Ocean off Point Mugu, California. These rockets will be held in readiness for the occurrence of flares producing S.I.D. Combined with the flare observations on Mt. Wilson, Sacramento Peak and Climax, these measurements should contribute substantially to our understanding of solar flares.

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Chemical Mechanism of Permanent Set

All available methods of imparting a permanent set to strained wool fibres are based on two consecutive intramolecular reactions, namely, disulphide bond breakdown, which promotes relaxation, and linkage rebuilding, which fixes the relaxed structure in its deformed state. The linkage rebuilding reactions are complex[1], but when a boiling 2 per cent solution of borax is used as the setting agent, one important reaction is that between the sulphenic acid, formed by breakdown of the disulphide bond, and the basic side-chains; set fibres contain fewer free lysine side-chains than untreated fibres[2], and fibres treated with 1-fluoro-2 : 4-dinitrobenzene acquire much less set than untreated fibres[3]. In striking contrast with these observations, it has now been found that the lysine side-chains play little, if any, part in the setting of strained fibres in a boiling solution of sodium bisulphite.

Lincoln wool fibres in the untreated state, after treatment with buffer and after treatment with 1-fluoro-2 : 4-dinitrobenzyl, were stretched 35 per cent, boiled for 30 min. in either a 2 per cent solution of borax or a 5 per cent solution of sodium bisulphite, and then released in boiling water. The amounts of set retained by the fibres after various times of release are shown in Table 1. Fibres treated with 1-fluoro-2 : 4-dinitrobenzene show such a marked contrast between borax and sodium bisulphite as setting agents as to leave no doubt that the main linkage-rebuilding reaction is different in the two cases. This deduction is supported by the fact that there is little difference between the amounts of free lysine side-chains in untreated wool and wool which has been set at 40 per cent extension in a boiling 1 per cent solution of sodium bisulphite for 1 hr. The set fibres were treated with 1-fluoro-2 : 4-dinitrobenzene and then hydrolysed, the ε-*N*-dinitrophenyllysine being isolated from the hydrolysate and estimated colorimetrically[2]. The lysine content of the set fibres was found to be 3·44 per cent, compared with 3·38 per cent for the untreated wool. In the light of these and earlier results[1], it seems clear that the rebuilding of disulphide bonds is the main cross-linking reaction when stretched wool fibres are set by means of a boiling solution of sodium bisulphite.

Table 1

Setting agent	Treatment of fibre	Percentage set after release in boiling water for: 5 min.	30 min.	60 min.
Borax	Untreated	28·3	26·6	26·3
	Buffer	24·1	22·0	21·4
	1-Fluoro-2 : 4-dinitrobenzene	18·6	12·8	11·9
Sod. bisulphite	Untreated	21·7	21·2	21·1
	Buffer	23·2	22·5	22·0
	1-Fluoro-2 : 4-dinitrobenzene	25·5	24·6	24·3

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An Approach to the Synthesis of Polycyclic Peptides

According to the cyclol hypothesis of peptide structure[1,2], intact peptides may have polycyclic structures which can be visualized, in part at least, as polycyclizations of peptide chains by means of [CO,HN to C(OH)—N] proton transfers. Here the H of the NH group of one residue is transferred to the CO-group of another: a N—C bond forms and the CO-group is replaced by a tertiary carbinol[3]. There result multiple as well as the CO–NH single peptide groupings. Fig. 1 shows some structures which emerge when consideration is confined to (1,6) and (3,7) transfers and piperazine and hydantoin rings are formed.

A molecule containing a multiple peptide or cyclol grouping has been synthesized[4], following the discovery of such a grouping in the peptide moiety of the ergot alkaloids[3]. This communication is concerned with the possible presence of such groupings in other synthetic molecules[5–8].

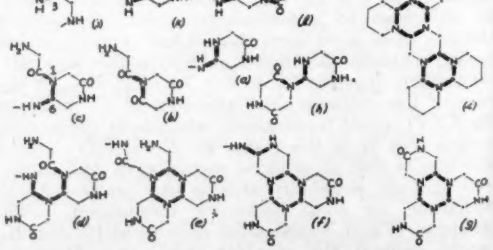

Fig. 1. Cyclized peptide chains, open and closed, in which · denotes CH*R* and o denotes C$^{\alpha}$(OH), and multiple peptide groupings composed of 2-, 3-, . . . 6-bonds uniting the residues are marked by heavy lines. Polycyclizations leading to cyclol-6 are depicted in (*c–g*), the structure cyclol-10 in (*i*). The diagrams indicate that hydrogen bridges would be possible in most cases, and that water molecules may be built in by means of hydrogen bridges, particularly in (*l*)

The new synthesis, achieved by a transannular interaction [CO,HN—CO to C(OH)—N—CO] across a cyclic keto-amide, shows that under appropriate stereochemical or geometrical conditions "amide bonds can exhibit a chemical reactivity greater than is usually attributed to them"[4], and indicates the impact on the experimental study of the cyclol hypothesis of the knowledge of transannular interactions between CO and *N*R groups now available[9]. Of signal importance is the fact that this transannular interaction, which changes a more or less flexible 10-atom ring into a rigid bicyclic structure, is spontaneous[4]. How is this to be reconciled with the fact that it replaces a CO-group by a high-energy tertiary carbinol? Presumably sufficient stabilization is conferred on the molecule by the interlocking rings. The case of the ergot tripeptides[3] prompted the same conclusion[2]. Here then are two structures which may be regarded as documents awaiting detailed interpretation. These and others now expected[2] will no doubt point the way to the systematic quantitative evaluation of stabilizing energies from this source, or perhaps to a broader view regarding the analysis of the heat content of molecules in which the multiple connexions between atoms in the structure play a leading part.

Multiple peptide groupings may be present in the recently synthesized hexaglycyl[5–7] and tetraglycyl[7] compounds. The monocyclic, closed peptide chain type of structure which is generally assumed for a peptide which has the composition nA—nH$_2$O, where A denotes an amino-acid molecule, is then supplemented by a number of possibilities which also merit consideration. Examples are shown in Fig. 1, (*b*,*k*) for $n = 3$, (*h*,*l*) for $n = 4$ and (*g*) for $n = 6$, (*i*) for $n = 10$. Some of these polycyclic structures are of special interest for these two compounds, because of features in the infra-red spectra[7] which so far lack interpretation.

Other findings also remain unexplained. One synthesis of the hexaglycyl compound was by way of an open polyglycine chain[5]. This was obtained by the polymerization of *N*-carboxy glycine anhydride in which, strikingly enough, an intermolecular proton transfer of cyclol type [CO,HN to C(OH)—N] is postulated. But taking the peptide chain structure as given, how is the ring closure to be explained? Why is the predominant product the hexa- and not the penta- or hepta- or other poly-glycyl? However, picturing not only inter- but also intra-molecular cyclol transformations leading to polycyclic structures, somewhat as the transformation of squalene to lanosterol[10,11] is visualized, a polycyclic structure would result, perhaps even cyclol-6, as depicted in (*c*–*g*). Alternatively, given the closed hexaglycyl chain, we can visualize 1, 2 or 3 separate (3,7) transannular interactions, or three related ones each facilitating the next and resulting in cyclol-6. The hexacyclic structure (*g*) with its kernel of a closed 6-bond multiple peptide binding would presumably have a considerable stability, perhaps increased by a trigonal circuit of OH··O hydrogen bridges or built-in water molecules or both. [The corresponding cyclol-10 structure is also of great interest. It is possible that this structure with its six additional asymmetric carbon atoms, or a partially decyclized form of it, might account for various properties of gramicidin-S so far unexplained (see R. Consden *et al.*, *Biochem. J.*, 41, 596; 1947). This decapeptide, for which two of the residues are assigned D-forms, has a high optical rotation; it is more resistant to hydrolysis than its partial hydrolysis products; these products do not include prolylvaline.] A cyclol-6 structure would explain the most striking 'anomaly' regarding the hexaglycyl, its resistance to acid and alkali hydrolysis. Whether this compound in fact has the closed peptide chain structure which has been assigned to it without discussion, or one or other of the various polycyclic structures, remains to be discovered. In view of the possibility of spontaneous polycyclizations, the tetraglycyl compound may also have a polycyclic structure, (*h*), (*l*), etc. All these issues can be studied also in relation to the unexplained features of the highly interesting infra-red spectra.

In this and other connexions, structures corresponding to (*h*) and (*g*) in which each CO—NH is replaced by CH_2—CH_2 would be of great value: they would permit the study of the multiple peptide bindings in the absence of CO—NH groupings, a matter of prime importance for interpretation of the spectra. They would have the composition of a valerolactam dimer and trimer. Replacing each CO—NH by CH_2—CH_2—CH_2 would give structures with the composition of the recently synthesized caprolactam dimer and trimer[5,12]. The possibility that these materials have polycyclic rather monocyclic structures merits consideration.

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Effect of Experimental Biliary Obstruction on the Phagocytic Activity of the Reticulo-Endothelial System

EXPERIMENTAL and clinical studies have frequently been reported on the effect of biliary obstruction and portal cirrhosis on the functions of the liver; but no reports on the influence of these conditions on the reticulo-endothelial system appear to have been recorded. The following investigations were therefore planned to study the effect of experimental biliary obstruction on the phagocytic activity of the reticulo-endothelial system.

The present experiments were carried out on male white rats of about 150 gm. body-weight. In thirty-eight rats the common bile duct was ligated under ether anæsthesia. These animals were then divided into five groups in which the phagocytic activity of the reticulo-endothelial system was measured at 3, 7, 16, 25 and 38 days respectively after the operation. Twenty-two intact males were used as controls. The phagocytic activity was measured by the rate of

Table 1. Shows Effect of Ligation of Common Bile Duct on the Phagocytic Activity of the Reticulo-endothelial System

Duration of biliary obstruction	No. of animals	Average weight gain in gm.	Weight of liver in gm. per 100 gm. of body-weight	Weight of spleen in gm. per 100 gm. of body-weight	Body-weight divided by weight of liver and spleen	Phagocytic index (K value)	Unit activity of reticulo-endothelial tissue (α value)
3 days	6	1	5·7	0·35	16·8	45	5·9
7 days	8	6	6·0	0·41	15·7	58	5·8
16 days	10	2	5·7	0·80	15·4	88	6·6
25 days	8	30	5·5	0·99	15·6	76	6·5
38 days	6	42	4·5	0·69	19·4	41	6·6
Control value in 22 animals			4·0	0·53	22·2	26	6·4

disappearance of a known amount of carbon from the circulating blood[1], since it has been estimated that about 90 per cent of the carbon is removed from the blood by the Kupffer cells of the liver, 3 per cent by the phagocytes of the spleen and the remainder by those of the bone marrow, lymph nodes and connective tissue. Each animal received one intravenous injection of carbon of particle size 250 A., the dose of the carbon being calculated on the basis of 8 mgm. per 100 gm. body-weight. Blood was then taken at short intervals over the period of one hour following the injection, and the concentration of the carbon in each blood sample was recorded by an absorptiometer. The logarithmic values of the absorptiometer readings lie on a straight line when plotted against time, and the slope of the line has been taken as the measure of total body phagocytic activity or the phagocytic index, which is denoted in the present communication by the symbol K. On the other hand, the phagocytic activity per unit of reticulo-endothelial tissue, here denoted by α, has been found to vary for a given dose of carbon according to the following equation[2]:

$$\alpha = \frac{\text{Total body-weight of animal}}{\text{Combined weights of liver and spleen}} \sqrt[4]{K}$$

All the animals were killed one hour after the carbon injection. In those in which the common bile duct was ligated it was found that the proximal part of the duct had become enormously dilated and contained a yellowish watery fluid. The body-weight and the weight of the liver and spleen were then recorded for each animal, and specimens of liver and spleen were fixed in Heidenhain's Susa fluid. Sections 10μ thick were cut and stained with weak eosin, or by a modified Masson's trichrome technique.

In a further group of ten animals the common bile duct was also ligated. These animals were kept for 42 days after operation, but after the thirty-eighth day they became increasingly ill and had to be killed.

The results are shown in Table 1. The weight of the liver is found to have increased by more than one-quarter by the third day after operation, the increase being maintained to the twenty-fifth day, after which it falls slightly. In the case of the spleen the weight falls during the first week, then rises to nearly double by the twenty-fifth day, after which it falls slightly. These changes are reflected in the ratio between body-weight and the combined weights of liver and spleen.

Following ligation of the common bile duct, the total body phagocytic activity (K value) becomes increased from 26, which is the average in the control animals, to 45 by the third day. The increase continues until the sixteenth day when it has risen to 88. Thereafter there is a gradual fall, although at the thirty-eighth day after operation the phagocytic activity still remains well above normal. In contrast the unit activity (α value) of the reticulo-endothelial tissue remains remarkably constant, which suggests that the increase in total body phagocytic activity (K value) may be due to increase in the amount of reticulo-endothelial tissue.

Microscopically, the liver parenchyma cells show surprisingly little degenerative change although small areas of necrosis may be found. The sinusoids are dilated and a progressive fibrosis develops around the portal vessels and gradually extends into the lobules. The Kupffer cells are filled with carbon; but throughout the groups of animals they show little variation from the controls in regard to number of cells and the amount of carbon phagocytosed, the appearances being in agreement with the relatively constant values of the unit activity of the reticulo-endothelial tissue (α value). In the spleen the capsule and the trabeculae become thickened, the Malpighian bodies less clearly defined and the reticulo-endothelial cells show increased activity as indicated by their number and the amount of carbon phagocytosed.

The above results show that when acute biliary obstruction is maintained an increased activity of the reticulo-endothelial tissue of the liver and spleen rapidly develops and continues for about five weeks, after which the animals become increasingly ill and die. The stimulation of the phagocytic activity may be due to the accumulation in the blood of steroids which are normally excreted or denatured by the liver[3,4]. This would be in agreement with the findings of Nicol *et al.*[5], who showed that natural and synthetic œstrogens stimulate the reticulo-endothelial system.

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Effect of Copper and Chelating Agents on Growth Inhibition of *Aspergillus niger* by 8-Hydroxyquinoline and Pyridine-*N*-Oxide-2-Thiol

When examining the toxicity of chelating agents, we have to deal with the toxicity of each of three entities, namely, the chelating agent itself, the unsaturated complex(es) of metal and chelating agent, and the saturated complex; for example, for copper and 8-hydroxyquinoline: free oxine, $CuOX$ and $CuOX_2$. Thus we found[1] for *Aspergillus niger* a minimal growth-inhibitory concentration of 50 p.p.m. for sodium dimethyldithiocarbamate, when the medium was free of heavy metals. The 1 : 2 complex with copper appears to be non-toxic, whereas the 1 : 1 complex is toxic. The activity of the 1 : 1 complex is abolished, however, when copper is removed from this complex by addition of a stronger, non-toxic chelating agent such as sodium dibutyldithiocarbamate.

Growth inhibition by two other chelating agents, namely, oxine (applied as the sulphate) and pyridine-*N*-oxide-2-thiol, and by their copper complexes has now been studied. For these a different pattern of toxicity was observed by Albert and co-workers for bacteria[2,3], and this was confirmed for the fungitoxic action of oxine by Block[4].

Shake cultures were made[5] of *Aspergillus niger* strain *M* in glucose mineral salts solution (*p*H 7·0), prepared with glass-distilled water and analytical grade reagents[1], and incubated for three days at 24°. The growth-inhibitory concentrations of oxine sulphate and pyridine-*N*-oxide-2-thiol were 0·2 and 0·5 p.p.m. respectively. In spite of many precautions, however, this shake medium was not completely free of heavy metals, as addition of 1 p.p.m. or higher concentrations of sodium dibutyldithiocarbamate, a stronger chelating agent than oxine or pyridine-*N*-oxide-2-thiol, decreased toxicity of these compounds to a minimal inhibitory concentration of 10 p.p.m. We assume that heavy metals drained from the glass wall were sufficient to form a toxic complex with oxine or pyridine-*N*-oxide-2-thiol. The copper content of the liquid medium itself was not greater than 0·01 p.p.m. (calculated as $CuSO_4.5H_2O$), as indicated by the yellow spores of *A. niger* strain *M* grown on agar media of the same composition[5].

$CuOX_2$ and the copper pyridine-*N*-oxide-2-thiol complex ($CuPTO_2$) are far more toxic than the chelating substances themselves. We found 0·1 and 0·2 p.p.m. as minimal inhibitory concentrations for these complexes.

Table 1 shows the effect of addition of copper to oxine sulphate and pyridine-*N*-oxide-2-thiol. Toxicity is first slightly increased, but with further increase of the copper concentration (as the 1 : 2 complex turns into the 1 : 1 complex) it decreases gradually. This decrease can be explained if we assume that a similar mechanism operates in *growth-inhibition* of *A. niger* by the copper complexes of oxine and pyridine-*N*-oxide-2-thiol as Albert and co-workers[2,3] have suggested for the *bactericidal* action of the corresponding ferrous iron complexes. The 1 : 1 complex is regarded by these authors as the actual toxic agent, but is supposed to enter the cells only very slowly. The 1 : 2 complex is believed not to be toxic as such, but to penetrate quickly into the cells and then to be transformed into the toxic 1 : 1 complex.

Table 1. Effect of Addition of Copper Sulphate on Growth Inhibition of *Aspergillus niger* by Oxine Sulphate and Pyridine-*N*-oxide-2-thiol

	p.p.m.	Min. inhibiting conc. (p.p.m.)	
		Oxine sulphate	Pyridine-*N*-oxide-2-thiol
$CuSO_4.5H_2O$	0	0·2	0·5
	0·03	0·1	0·2
	0·1	0·1	0·2
	0·3	0·1	0·5
	1	0·2	0·5
	3	0·5	0·5
	10	0·5	0·5

It is possible to calculate the approximate amounts of 1 : 2 and 1 : 1 complexes, when copper and oxine are present together, if the solubility of $CuOX_2$ and the equilibrium constant K of the reaction

$$Cu^{2+} + CuOX_2 \rightleftarrows 2\,CuOX^+$$

are known. Log K was determined spectrophotometrically and found to be 0·8.

These calculations were carried out for the culture media, making allowance for the occurrence of $CuOH^+$. $CuOX_2$ is never precipitated in these solutions, since the solubility was found to be about 1 p.p.m. The results (not reproduced here) indicate that, when copper is present in excess, concentrations of $CuOX_2$ exceeding 0·03 p.p.m. are toxic, whereas no correlation is found with the amount of 1 : 1 complex present. This suggests strongly that the concentration of 1 : 2 complex is decisive for toxicity. Only when $CuOX^+$ occurs in amounts equal to or more than 0·5 p.p.m. (namely, in the combination of 0·5 p.p.m. oxine sulphate and 10 p.p.m. $CuSO_4.5H_2O$ where only 0·02 p.p.m. $CuOX_2$ is present) it seems to have a definite effect of its own.

When there is less copper than required to convert all oxine in the medium into $CuOX_2$ (that is, when excess oxine is present) the correlation between toxicity and amount of $CuOX_2$ present does not apply. It seems as if, inside the cells also, an excess of copper must be present in order to transform $CuOX_2$ into $CuOX$ there.

Ferrous sulphate was found less effective than copper sulphate.

Our growth results with oxine agree more or less with those of Block[4] and of Anderson and Swaby[6].

Growth inhibition of *A. niger* by oxine and pyridine-*N*-oxide-2-thiol seems to differ from that effected by sodium dimethyldithiocarbamate in so far as the 1 : 2 complexes of the former are toxic whereas that of the latter is not[1]. The most plausible explanation for this fact is the difference in solubility in water of the three 1 : 2 complexes. These have been estimated to be about 1, 0·2 and 0·01 p.p.m. respectively (unpublished work). Obviously, the toxic concentration for *A. niger* can be reached in aqueous solution by $CuOX_2$ and by $CuPTO_2$ but not by the saturated complex of copper dimethyldithiocarbamate ($CuDDC_2$). So that in the latter case toxicity is determined only by the amount of CuDDC present.

This hypothesis is substantiated by the observation that there are certain more sensitive moulds which are inhibited by the small amounts of $CuDDC_2$ which dissolve. These moulds show in fact an inhibition pattern by sodium dimethyldithiocarbamate and its copper complexes which is so similar to that of oxine and pyridine-*N*-oxide-2-thiol and their complexes that the same principles appear to underly toxicity.

This pattern will be followed in general if the solubility of the 1 : 2 complex exceeds the minimal inhibiting concentration for the organism involved. If the solubility is lower, the pattern of *A. niger* treated with sodium dimethyldithiocarbamate can be expected.

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State of Copper in Polyphenoloxidase (Tyrosinase)

It was shown in the classical work of Kubowitz[1] and of Keilin and Mann[2] that the prosthetic group of polyphenoloxidase is constituted by copper. Since then it has been unanimously admitted that the metal of the enzyme is in the bivalent state and that the catalytic activity of the enzyme is based on the change of valency cupric ⇌ cuprous, as follows (for excellent discussions, see refs. 3,4) :

(1) $2\,Cu^{2+}$-enzyme + *o*-dihydroxyphenol = $2\,Cu^{+}$-enzyme + *o*-quinone + $2H^{+}$

(2) $2\,Cu^{+}$-enzyme + $\frac{1}{2}O_2$ + $2H^{+}$ = $2Cu^{2+}$-enzyme + H_2O

o-dihydroxyphenol + $\frac{1}{2}O_2$ = *o*-quinone + H_2O

The substrate is oxidized by losing two electrons and two protons. The two electrons are taken by the copper of the enzyme which thus passes to the cuprous state. The cuprous enzyme rapidly transfers the two electrons to oxygen (which immediately forms water with the two protons liberated) and so returns to the cupric state ready to recommence the catalytic cycle.

This certainly very attractive hypothesis is based on ingenious but indirect evidence. Kubowitz[1] has shown that potato-polyphenoloxidase takes up 0·5 *M* carbon monoxide per mole copper only in the presence of the substrate and that the bound carbon monoxide is liberated again by adding hydrocyanic acid. It must be remembered, however, that this experiment is a difficult one: the low copper content of the enzyme makes the use of a large amount of protein (85 mgm.) for each Warburg flask unavoidable; nevertheless, the pressure differences read on the manometers remain relatively low[3] (11 mm. and 12 mm.). Moreover, the purity of the enzyme used by Kubowitz[1] was only 65 per cent of his best preparation, which appears to have a rather low specific activity[3].

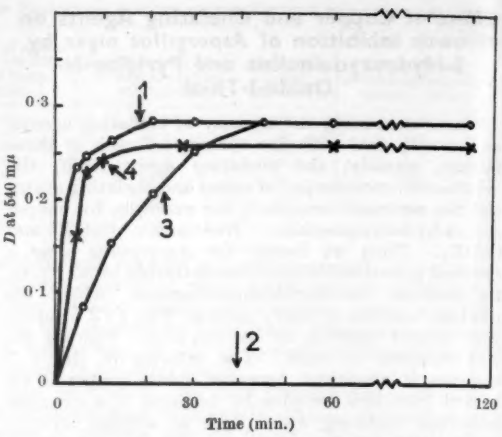

Fig. 1. Course of the reaction between copper and 2.2′-diquinolyl in acetic acid. Curve 1: 11·5 μgm. cuprous ions in 2 ml. 0·05 *M* phosphate buffer pH = 6·8, to which, at zero time, 1 mgm. of 2.2′-diquinolyl in 2 ml. of glacial acetic acid was added. Curve 2: 11·5 μgm. cupric ions in the same buffer and the same reagent. Curve 3: as in curve 2, but at zero time hydroxylamine sulphate (in crystals) was added. Identical results were obtained with copper previously incubated with 5·53 mgm. of bovine serum albumin and with conalbumin. Curve 4: 5·32 mgm. polyphenoloxidase containing 10·6 μgm. copper (as determined by an independent method (ref. 9)) in the same buffer and reagent as in curve 1

The recent preparation of mushroom-polyphenoloxidase which appears homogenous by electrophoresis and by ultracentrifugation[5], and the discovery of a reagent specific for cuprous copper by Hoste[6] has made possible a direct examination of the state of copper and of any valency change during its catalytic activity.

The time course of the reaction of copper with 2.2′-diquinolyl in glacial acetic acid (as measured with a Beckman *DU* spectrophotometer at mμ 540) is shown in Fig. 1. On the addition of the reagent to cuprous copper the characteristic colour appears immediately and after a short period of increase remains stable (curve 1); the later addition of a reducing agent (hydroxylamine sulphate[6]) does not have any influence on the colour. Cupric copper does not react at all (curve 2). If to a solution of cupric copper + 2.2′-diquinolyl in glacial acetic acid a reducing agent is added, the colour develops and stabilizes relatively slowly (curve 3). The same measures were repeated in presence of copper incubated with crystalline bovine serum-albumin and with conalbumin, two proteins which bind copper strongly[7]. The course of the reaction and the value of the absorption remained identical, showing that the glacial acetic acid of the reagent separates the copper efficiently and quantitatively from these proteins[8].

The reaction of polyphenoloxidase with the reagent, in air and in the absence of substrate, is shown by curve 4. The presence of substrate (2, 1, 0·5 and 0·25 *M* catechol per mole copper) in anaerobic conditions under argon or under carbon monoxide and the previous or later addition of a reducing agent have not the least influence on the time course or on the value of the absorption.

It is evident that the copper of the polyphenoloxidase is always in the cuprous state. The alternative explanation of these results, namely, that the copper is reduced to the monovalent state by a hypothetical

reducing agent belonging to the apoenzyme after it has been separated from its specific site by the acid of the reagent, is excluded by the time course of the reaction.

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Biological Role of Potassium-40

MY investigations on the effect of potassium-40 upon the growth and development of *Aspergillus niger* lead to the conclusion that the radioactivity of potassium-40 (normally present in organisms) is not connected with any biological reaction or process proceeding under normal conditions in plants and animals[1]. Direct confirmation by experimental observations on the influence of potassium-40 upon animals was not given.

For this purpose, the isolated heart of a frog has been perfused with physiological Ringer solutions containing: (1) ordinary potassium; (2) potassium enriched in potassium-40; and (3) potassium impoverished in potassium-40.

The isotopic composition of the potassium was the same as that used in my earlier work[1]. The enriched potassium contained 1·34 per cent of potassium-40 and the impoverished 0·0002 per cent of potassium-40. The composition of the physiological Ringer solution was: sodium chloride 0·60, potassium chloride 0·01, calcium chloride 0·01, sodium bicarbonate 0·02 and water 100·0.

The Ringer solution was passed through the sinus venosus and the right aortic arch of the isolated heart of autumn frogs (*Rana ridibunda*). The pulmonary vessels and the left aortic arch were ligatured. The cannulas were joined to the isolated heart apparatus with the aid of thin glass sections. During the experiment the venous and arterial pressures were maintained at constant levels.

The contractions and the amplitudes of the heart action were recorded as usual on a kymograph.

The perfusion solutions were alternated in various combinations: (1) solutions containing ordinary potassium; (2) solutions not containing potassium; (3) solutions containing potassium enriched in potassium-40 and (4) solutions containing potassium impoverished in potassium-40. After each perfusion, the solution in the system was carefully flushed out by the next solution to ensure that the effect observed was due to the last solution only. During the perfusion of the solution with no potassium the heart stopped beating.

Table 1

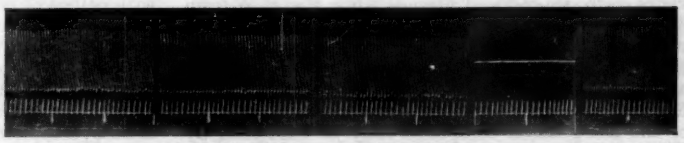
Perfusion solution	Volume of solution (cm.³)	Time of perfusion (min.)	Heart action		Time of restoring the heart action (min.)
			No. of contractions/min.	Amplitude (mm.)	
Ringer	—	4	60	10	
Ringer with potassium-39	54	13	60	10	
Ringer with potassium-40	50	14	57·6	9·6	
Ringer without potassium	15	16·5	Stop for 15 min.		
Ringer with potassium-39	40	16	56·4	10·6	7·5
Ringer without potassium	14	10·5	Stop for 5·5 min.		
Ringer with potassium-40	54·5	17·5	57	11·6	6
Ringer	52	14	53·8	13·0	
Ringer with potassium-39	45	12	52·6	13·0	

Parts of the kymograph records obtained in one set of experiments are given in Fig. 1. Statistical analysis of the experimental results has shown that a successive and repeated change of Ringer solutions with an equal content of potassium-40 does not change either the number of the heart contractions or the contraction amplitude.

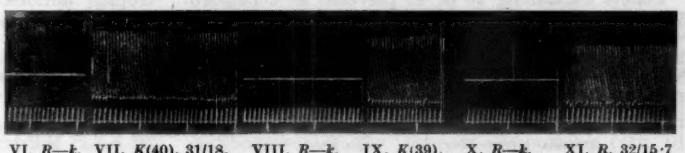
The results of another experiment are given in Table 1.

The character of the heart muscle contraction depended only on the concentration of the total potassium and did not depend on the content of potassium-40 in it, although the range of the potassium-40 content was changing by approximately 6,700 times.

Direct proportionality between the concentration of the total potassium and the growth of *Aspergillus niger* or the action of the heart muscle was found in all the experiments. There is no such relation between the content of potassium-40 in the potassium of the



I. *R.* 36/16·5. II. *K*(39). 32/16. III. *K*(40). 30/16. IV. *R—k.* V. *K*(39). 36/17



VI. *R—k.* VII. *K*(40). 31/18. VIII. *R—k.* IX. *K*(39). 30/17. X. *R—k.* XI. *R.* 32/15·7

Fig. 1. Kymograph record of frog's heart perfused with various solutions. *R*, Ringer solution with ordinary potassium; *R—k*, Ringer solution without potassium; *K*(39), Ringer solution with potassium-39; *K*(40), Ringer solution with potassium-40. The numerals 36/16·5 . . . give the average number of heart contractions in 1 min. followed by the average amplitude of contractions in mm.

Ringer solution (that is, the isotopic potassium composition) and the growth of *Aspergillus niger* or the heart action. Thus the biological role of potassium seems to be related only to its chemical properties and its concentration. As to the radioactivity of potassium-40 (and probably the radioactivity of all the other naturally radioactive chemical elements) normally occurring in all organisms, in the process of phylogenesis the organisms do not appear to have made biological use of the natural radioactivity. Consequently, the radioactivity of potassium-40 does not affect any of the normal biological reactions or processes in organisms; in other words, organisms do not need the radioactivity of potassium-40. The possibility may even be imagined of the existence of a living organism using only potassium-39 completely free of potassium-40. Nevertheless, the tissues of organisms have evidently got a certain resistance against increase in potassium-40 in the medium and against its radiation. With heavy doses of potassium-40, the reaction of organisms is only of a pathological, destructive, negative character.

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Resistance of 9-α-Fluoro-11-β-Hydroxyprogesterone to Inactivation in the Liver of the Guinea Pig

The property of 11-β-hydroxyprogesterone in preventing œstrogen-induced fibroids or œstrogen-induced uterine growth is greatly enhanced[1] by introduction of fluorine at C(9) (Fig. 1); the same applies to the antiluteinizing potency (unpublished work). The anticœstrogenic potencies of the halogenated 11-β-hydroxy compound are possibly even greater than those of progesterone.

The question is which physiological mechanism effects this increase of potency?

Progesterone has long been known to be inactivated in the liver; an amount of progesterone which prevents œstrogen-induced fibroids when absorbed from a subcutaneously implanted pellet loses this property when absorbed from a pellet implanted into the spleen[2] (summary, ref. 4). Thus the question arose whether the considerable increase of the antifibromatogenic and other anticœstrogenic potencies of the halogenated compound may be explained by a greater resistance to intrahepatic inactivation in comparison with progesterone.

Pellets containing 20 or 40 per cent of 9-α-fluoro-11-β-hydroxyprogesterone mixed with 80 or 60 per cent of cholesterol were implanted into the spleen of castrated female guinea pigs. Pellets of œstradiol were implanted subcutaneously. The animals were killed and examined three months later. Results are summarized in Table 1; they should be compared with those in Table 1 of our former communication[1] with subcutaneously implanted pellets of the halogenated compound.

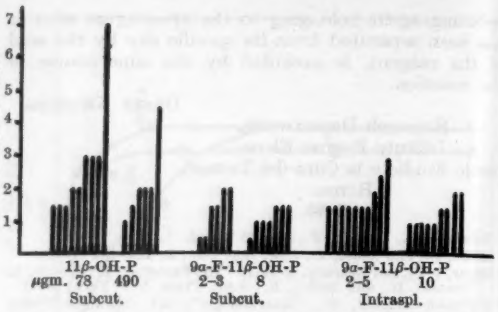

Fig. 1. Comparative antifibromatogenic action obtained with 11-β-hydroxyprogesterone and 9-α-fluoro-11-β-hydroxyprogesterone. Each column corresponds to one animal of the respective group; height of the column-units of our scale of classification of the fibrous tumoral effect[2]. The antifibromatogenic action was greater with 2–3 μgm. of the halogenated compound than with 490 μgm. of the non-halogenated one[1]. Little is changed when the halogenated compound is absorbed from an intrasplenic pellet instead of a subcutaneous one

The halogenated compound is active even when absorbed from a pellet implanted into the spleen (Fig. 1). With such small quantities as 3–10 μgm. a day absorbed from an intrasplenic pellet a very pronounced antifibromatogenic and antihysterotrophic action was manifest, only slightly inferior to that with a subcutaneous pellet of the same compound. Instead of an average fibrous tumoral effect of 4 or more, as obtained in experiments with œstradiol alone, averages of 1·8 and 1·3 were obtained when the halogenated compound was absorbed from an intrasplenic 20 or 40 per cent pellet. Even when making an ample allowance for those great variations which occur in similar experiments with the production and prevention of abdominal tumours, the conclusion is justified that the greater part of the halogenated compound reaches the general circulation without being inactivated in the liver.

A detailed comparison with our former experiments with pellets of progesterone implanted into the spleen is very impressive. Whereas such small quantities as 3–10 μgm. of the halogenated progesterone derivative pass through the liver almost entirely without alteration (Fig. 1), quantities of progesterone as large as ten or even thirty times that are completely, or almost completely, metabolized by the liver (see Group III*a*, ref. 2).

Besides the antifibromatogenic and antihysterotrophic actions, other properties characteristic of progesterone also were manifest in the present work

Table 1. Experiments on Nineteen Castrated Female Guinea Pigs Receiving during Three Months Œstradiol Absorbed from Subcutaneously Implanted Pellets, and 9-α-Fluoro-11-β-Hydroxyprogesterone Absorbed from Pellets Implanted into the Spleen

Group	No. of animals	Œstradiol (μgm./day)	9-α-Fluoro-11-β-hydroxyprogesterone (μgm./day)	Fibrous tumoral effect (average)§	Coeff. Q_{3-5}‡	Uterine weight (gm.)	No. of pellets and concentration of 9-α-fluoro-11-β-hydroxyprogesterone
A	10	45*–87 (55)	3·4 (1–5)	1·8	0·1	1·9 (1·0–3·1)	1 : 20 per cent
B	9	32†–80 (51)	10·0 (7‡–18)	1·3	0	1·8 (0·9–2·9)	1 : 40 per cent

* 1 animal with 16 μgm. only. † 1 animal with 13 μgm. only. ‡ 1 animal with 3 μgm. only. § For technical explanations, see refs. 3 and 4.

with intrasplenic pellets of the halogenated progesterone derivative. Thus the vagina, which opens under the action of œstrogen, was found closed in eighteen out of nineteen animals; the symphysis pubis opened in five out of nine animals in group *A*, and in eight out of nine animals of group *B*.

There is one objection against the conclusion that the fluoro derivative resists inactivation in the liver. In six out of nineteen animals of groups *A* and *B* there were considerable adhesions between the spleen and the abdominal wall. The question of adhesions in similar experiments has been already discussed on former occasions[3]. It is not likely that results have been stultified by the adhesions.

Thanks are due to Dr. Josef Fried, of the Squibb Institute for Medical Research, New Brunswick, N.J., for 9-α-fluoro-11-β-hydroxyprogesterone; and to Dr. E. Mardones for assistance in examining the experimental animals.

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Influence of Hexanitrodiphenylamine on the Incidence of Neoplasms in the Mammary Tissue of Rats

The industrial use of hexanitrodiphenylamine (dipicrylamine, DPA) and the consequent exposure of human beings to it led us to study the chronic toxicity of this compound in experimental animals. In the first part of a feeding test with rats, the substance appeared to have a low toxicity; but towards the end of the life-span of animals which were fed with large amounts of hexanitrodiphenylamine, we were surprised to see that all the females had big and often multiple mammary tumours. These were fibroadenomata, adenofibromata or fibromata of the same type as occur spontaneously in our rats.

In a following experiment, hexanitrodiphenylamine was given to rats, directly after weaning, in a dose of 500 p.p.m., mixed into the dry food. The diet consisted of two-thirds whole wheat meal, one-third of whole dried milk powder and 1 per cent of a salt mixture. Food and water were given *ad libitum*; green fodder twice a week was included. The rats were derived originally from the Wistar strain, but brother–sister mating has not been continued during the past ten years.

Since hexanitrodiphenylamine can easily be hydrolysed *in vitro* to picric acid and picramide (2,4,6 trinitroaniline), these substances were included in the test, using the same dosage of 500 p.p.m. At the end of about two and a half years, the experiment had to be terminated. In the male animals, at autopsy, a variety of tumours were observed. Since neither incidence nor type of these was different from the usual, these animals will not be considered further. In the females, the occurrence of tumours of the mammary glands in the different groups is summarized in Table 1.

Table 1. Incidence of Mammary Tumours in Female Rats treated with Hexanitrodiphenylamine.

Dosage, 500 p.p.m. mixed in dry food. The numbers in brackets indicate total incidence as found in our untreated rats, including that from other experiments during the same period; see text

Group	No. of rats	No. alive after 2 years	No. with mammary tumours	Average age of appearance of tumours
Controls	19 (83)	12 (63)	8 (18)	25 months (26)
Hexanitro-diphenylamine	10	1	10	19 months
Picric acid	10	7	4	22 months
Picramide	8	7	4	29 months

From these observations it appears that in all groups the incidence of mammary tumours is high. Since in the control rats the frequency is much higher than we are accustomed to see, we have included (in brackets) the corresponding data from other untreated control rats, collected from a series of different toxicity experiments in this laboratory with the same kind of rats and diet and from the same period.

Again, in this experiment, every female rat treated with hexanitrodiphenylamine developed mammary tumours, which were found at autopsy, on the average at the age of nineteen months, which is equal to the life-span. In the control rats the age of the appearance of tumours is twenty-five to twenty-six months, and the average life-span more than twenty-five months (nearly half the animals were still alive at the end of the experiment). Without exception all these tumours, including those of the untreated animals, were fibroadenomata, adenofibromata or fibromata. All transplantation experiments with these tumours gave negative results. Another interesting feature is that the rats treated with hexanitrodiphenylamine nearly all had multiple tumours, with an average of three per animal. In control rats the development of more than one mammary tumour in the same animal is seldom observed. Hence this substance seems to increase the rate of formation of this type of tumour in our rats to the level of 100 per cent. Since the spontaneous occurrence is already rather high, it is doubtful whether hexanitrodiphenylamine should be considered on this evidence to be a true carcinogen. In another feeding test, but with mice (F_1 generation of *DBAf* × 0 20) and the same dosage (500 p.p.m.) no effect has been observed within a period of 21 months. This experiment is still going on.

In the case of picric acid and picramide, the incidence of mammary tumours did not differ from the control animals. It seems, therefore, that these products are not responsible for the effect observed with hexanitrodiphenylamine. Rats treated with high dosages of hexanitrodiphenylamine soon develop a purple-brown discoloration of nearly all tissues, and especially of the testis. This effect was not observed with either picric acid or picramide. The composition of the pigment responsible for the discoloration is now being examined.

Our thanks are due to N. V. Mekog, Ymuiden, The Netherlands, and Dr. G. J. H. Ebbinge Wubbe for their help and interest in this work. We also wish to thank Dr. O. Mühlbock, of the Ant. v.

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Copulation in Calanoid Copepods

In the course of picking out copepods for experiments on their feeding, respiration and other activities, I have made a number of incidental observations on the manner of pairing in four species of calanoids (*Centropages hamatus*, *Temora longicornis*, *Eurytemora velox* and *Acartia clausi*), which so far as I know has previously been described in only one calanoid genus, *Diaptomus*[1].

In all these species the process of pairing and the transfer of the spermatophore are substantially the same. Whether the male actively pursued the female could not be determined; the first event in the pairing which was observed was the seizing of the female by the male. This was done by the same means and in nearly the same position on every occasion when it was observed. The male held the female around the terminal segment of the urosome or the caudal rami by means of the right geniculate antennule, the distal part of which was so far reflected that its terminal segments crossed the middle region of the antennule. A pair of copepods might remain for several minutes in this position, and two specimens of *Eurytemora* remained paired in this position for several days in a watch-glass. As the pair swam about, the female made vigorous efforts to shake off the grip of the male and often succeeded. For his part, the male attempted to exchange his first grip for a second in which he gripped the female either immediately in front of, or immediately behind, the genital somite by means of the fifth leg or legs (figures of the fifth legs of these species are given by Sars[2] and Gurney[3]). In most pairs the two animals were lying head to tail with their ventral sides opposed, but in one pair of *Centropages* both had their dorsal sides uppermost and their urosomes so flexed that the genital somites were opposed. *Centropages* habitually holds its urosome dorsally flexed, so that this is a more natural position for copulation in this genus than might at first appear. Since the animals often 'danced' violently in this phase of pairing, it was sometimes difficult to make out the exact position in which they were lying.

It was very easy to determine whether or not a spermatophore had been transferred after the pair had separated, since the spermatophore attached to the genital somite is easily seen; but the actual transfer was never observed. Sars[2] says of *Euchaeta*, and Wolf[1] of *Diaptomus*, that the spermatophore is placed on the genital somite of the female by the left fifth foot of the male. I found some preserved males of *Euchaeta norvegica* in which the minute forceps at the tip of this leg was fixed holding the neck of a spermatophore, but I was unable to determine whether a forceps of this kind was used in the species I observed.

Females examined after copulation had only a single spermatophore attached to the genital somite, and usually the pair separated after this second stage of copulation. In a few cases, however, after transfer of the spermatophore the male reverted to his first grip, holding the urosome of the female with his antennule. Although it was never actually seen, it is possible that a male might clasp the female again with the fifth leg and transfer a second spermatophore—three or four are occasionally found on one female.

The species in which pairing has been described belong to widely separated families of the Calanoida[2,3], but the course of events in pairing is identical in all of them. It seems reasonable to infer that pairing follows very much the same course in all the calanoids which have geniculate antennules and clasping fifth legs, that is, in twelve of the twenty-six families into which Gurney[3] divided the Calanoida. In most of the remaining families the fifth legs clearly form a clasper which, like that of *Euchaeta*, is used in the way suggested above, but the antennule is not geniculate and we have no information about the first phase of copulation.

The observations described were made in the Zoology Department, University of Southampton, and in the Marine Station, Millport.

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Growth of Fowl Plague Virus in Macrophages and Giant Cells

The fowl plague virus strain 'Rostock' has been grown in monolayer tissue cultures of macrophages. The virus grows rapidly in these cells, producing characteristic cytopathogenic effects.

The macrophage tissue cultures were prepared from heparinized whole chicken blood which was centrifuged to isolate the white blood cells. After 18–24 hr. incubation the cultures are thoroughly washed with Earle's saline, which removes any remaining red blood cells since only the leucocytes firmly adhere to the glass. The nutrient medium throughout this work consisted of 20 per cent chicken serum, 5 per cent embryo extract, and 75 per cent Earle's saline. The Earle's saline was buffered to maintain a *p*H in the carbon dioxide incubator between 6·5 and 7·0.

The growth of fowl plague virus on 4–5-day old macrophage monolayers has been studied by measuring the release of new infectious particles into the medium, the infectious units being assayed by the Dulbecco plaque technique on chick embryo fibroblasts[1]. After a latent period varying between 1 and 2 hr., there is an exponential release of virus which lasts between 3 and 4 hr. The cells are drastically altered during this time. Already after 1 hr. they cannot be stained by the vital dye, neutral red. Between 6 and 8 hr. after the start of infection, practically all the cells have rounded up and are destroyed.

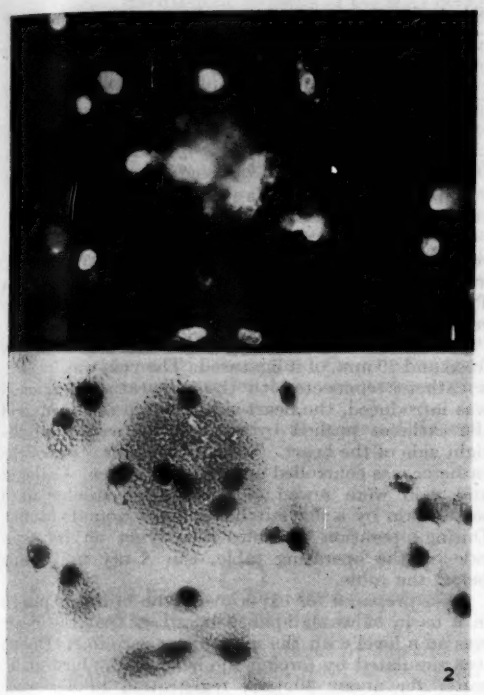

Fig. 1. Macrophage culture 2–3 hr. after infection, fixed in alcohol at − 50° C. and stained with fluorescent antibody to *g*-antigen

Fig. 2. The same culture stained by the Wright–Giemsa technique

Fluorescent-labelled antiserum to "gebundenes Antigen" (*g*-antigen)[2,3], the viral component containing all of the ribonucleic acid as well as some of the protein, would be expected to react with closely related components in the infected cell, namely, the soluble antigen (*s*-antigen)[3,4]. Fluorescein-labelled antibody to *g*-antigen was used according to the Coons's technique[5], and showed that between 1 and 2 hr. after the start of infection a detectable amount of new *s*-antigen is concentrated in or around the nucleus. This is similar to the picture obtained by Breitenfeld when the fowl plague virus is grown on monolayers of chick embryo fibroblasts[6] and is also related to the localization of influenza *s*-antigen in the nuclei of cells of the respiratory epithelium of infected ferrets[7].

Particular attention was focused on the giant cells which typically arise in our macrophage cultures. Giant cells infected with fowl plague can still be stained with neutral red up to 4–5 hr. after the start of infection, but they present the typical picture described above when treated with the fluorescent antibody. Indeed, in the multinucleated giant cells all the nuclei fluoresce. This is illustrated in Fig. 1, which shows an infected macrophage culture containing a giant cell, a young giant cell (dinucleate) and several normal macrophages. Fig. 2, showing the same cells stained with the Wright–Giemsa stain, demonstrates the identity of nuclei and centres of *s*-antigen localization.

Fowl plague virus multiplies in the macrophage cultures in a virulent fashion. This means a rapid production of new virus with an accompanying cytopathogenic effect, similar to the growth in chick embryo fibroblasts. Recent observations of Barski[8] suggest that the polio virus, Mahoney type I strain, multiplies in tissue cultures of monkey macrophages without the typical cytopathogenic effects which characterize the multiplication of polio on monkey epithelial cells[9]. Furthermore, the polio virus continues to be released from the macrophage cultures as long as eight days after the start of infection, whereas polio virus grown in monkey epithelial cultures is released during a period lasting 4–8 hr. after infection[10]. Thus the macrophage cell can react with virus to produce a 'virulent' type of infection in which the virus multiplies rapidly, or a 'latent' type of infection with a slow and continuous viral growth.

Details of these investigations will be published elsewhere.

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Tübingen.
April 9.

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Honey Bees in Southern Nigeria

The recent paper by Dr. M. Lindauer on swarm-bees searching for a new home[1] leads me to record some observations on honey bees (*Apis mellifica* L., var. *adansoni* × var. *unicolor*) in the forest region of southern Nigeria which suggest that honey bees there have a preference distinctly different from European bees for their nesting site.

A swarm was placed temporarily in an old box made from 3-mm. plywood which was put on a large lawn in the shade of a dense tree, but exposed to all winds. Twice the swarm flew out and was brought in again. When it was brought in for the third time, the front of the 'hive' was removed and now the swarm stayed, producing and partly filling 10 dm.^{2} of honeycomb in three days. The swarm was then transferred to a proper hive, which it left next morning. The rear end of the inner top cover was then removed, thus letting a draught through the entire hive; the swarm was brought in again the same evening and it now accepted the hive.

Six wild nests of honey bees have been observed. Five of them were in hollow trees, and in each of them the cavity was chimney-shaped, open at both ends with the combs in the lowermost part. Three of these trees were on large clearings exposed to winds from all directions and the nests were placed 2–5 m. above the ground. The other two were in high bush with rather dense undergrowth, and were

15–20 m. above the ground, where the crowns were relatively open. In all these nests the entrance was shaded by the foliage overhead, but there was no (or in one case a very little) foliage in front of them. One nest was inhabited for two and a half years, but when a strangler fig grew up along the trunk and hid the entrance behind dense foliage the nest was abandoned. All these trunks abounded with ants of various species, but evidently this did not cause the bees any inconvenience. The sixth wild nest was situated under the roof of a three-story building, just inside the eaves, the combs hanging down from the roofing sheets (corrugated asbestos cement), the entrance exposed to all winds and the roof baked by the sun during the whole of the day, the inside of the roofing sheets often reaching temperatures above 40° C. There is, however, a steady, fairly strong draught from the eaves towards the ridge. The solidifying point of the wax from this nest qroved to be slightly higher than that normally puoted[2], namely, 60·5–62° C.

Wax from uppermost cm. of three oldest combs	63·0°–64·3° C.
Wax from uppermost cm. of three youngest combs	63·5°–65·0° C.
Wax from lower parts of three oldest combs	61·8°–62·5° C.
Wax from lower parts of three youngest combs	62·7°–63·5° C.
Commercial Beeswax, white (B.D.H.)	60·7°–61·8° C.

In contrast to the preference of the European honey bees for a site sheltered from all winds and for a confined space, the south Nigerian honey bees seem to prefer a site exposed to the winds, and natural ventilation through the nest seems to be essential. I have never observed any preference for wind-exposed sites or for rooms with draught through them by any of the other social bees in this country. It would be most interesting to have further observations from other parts of the tropics.

J. Birket-Smith

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Ibadan.
April 2.

[1] *Nature*, **179**, 63 (1957).
[2] "Handbook Chem. and Phys.", 33rd edit., 1320 (1951–52).

Right-Ventricle Pressures and Heart-Rate in Diving Birds

For these experiments the following sea-birds and water-fowls were used: shag, razorbill, guillemot, black guillemot, puffin and eider. The birds were caught in the bird colonies on oceanic cliffs at Runde, near Aalesund, on the west coast of Norway.

For electrocardiograms, an 'Elema Junior' cardiograph fitted with special electrodes, adapted for this purpose, were used. The electrodes were applied to the left thigh and right side of the thorax. The ventricle pressures were measured electrically by way of a condenser manometer connected to a Tybjerg–Hansen amplifier leading to the electrocardiograph.

On the left side of the neck the vena jugularis was freed and 20 mm. of it ligatured. The vein was incised, a catheter connected to the apparatus mentioned was introduced, the heart-side ligature removed, and the catheter pushed by way of the vein into the right side of the heart. The proper application of the catheter was controlled on an X-ray screen. Catheter and vein were sewed together, the catheter fixed to the skin by a few stitches and the wound sutured. During operation the bird was lying on its right side on the operating table, the X-ray unit being below the table.

Thus prepared for experiment, the bird was placed in a basin of water upon a board so that the heart was on a level with the manometer zero-line. Diving was simulated by forcing the head of the bird under water for about 30 sec., registration taking place from the very beginning of diving, or at least from the middle of this period. In all, three periods were registered: (1) immediately before diving, (2) during diving, and (3) immediately after diving. The experimental sequence was repeated after an interval of 2–3 min. Before each of the three steps of experiment, the zero-line and the adjusting pressure (usually 500 mm. water) were measured.

Table 1. Heart-beats/Sec. Before, During and After Diving

Species	Heart-beats/sec. Before dive	During dive	After dive	Percentage decrease during dive	No. of exp. birds (and readings taken)
Puffin	7·3	3·0	8·4	59	20 (39)
Guillemot	4·5	1·8	5·6	60	15 (28)
Shag	3·1	1·25	2·9	60	10 (18)
Eider	2·8	1·3	3·1	53·5	2 (4)
Razorbill	3·7	1·4	5·3	62	2 (4)
Black guillemot	5·7	3·3	6·2	42	2 (4)

Table 1 shows the heart-beats/second before, during and after diving for 51 birds examined. As will be seen, the mean decrease in heart-rate during diving is more than 50 per cent (except in the black guillemot) and mostly about 60 per cent. The highest normal rate before diving was found in the puffin, and the lowest in the eider. Thus there seems to be an inverse proportionality between heart-rate and body-size. Medium-size birds showed heart-rates between these limits. Shags and eiders are about the same size and have the same heart-rate. The rate after diving is somewhat higher than before diving.

Table 2 shows the results from the pressure experiments. Diastolic and systolic pressures were also measured before, during and after diving, and the stroke pressures in the corresponding periods were calculated. These pressures seem to be nearly the same before, during and after diving. There seems further to be some relation between heart-pressure

Table 2. Diastolic and Systolic Pressure in Right Ventricle Before, During and After Diving. The Pressures are given in mm. Water

Species	Diastolic pressure Before dive	During dive	After dive	Systolic pressure Before dive	During dive	After dive	Stroke pressure Before dive	During dive	After dive	No. of exp. birds (and readings taken)
Puffin	130	105	126	356	370	360	242	264	239	3 (26)
Guillemot	119	147	102	327	360	366	208	212	246	7 (49)
Black guillemot	85	91	115	288	315	278	203	224	163	3 (25)
Shag	335	348	325	598	640	578	246	289	254	13 (98)

and body-size, the diastolic pressure of the shag being nearly as high as the systolic pressure in the smaller birds. There is no increase in pressures after diving in the birds examined (except diastolic pressure in the black guillemot).

Considering the relation heart-pressure/heart-rate, as appears from Tables 1 and 2, there is no decrease at all in the heart-pressure during diving, although the heart-rate is reduced to about 60 per cent of that before diving. I may also direct attention to the fact that the slightly increased heart-rate after diving (compared with the rate before diving) does not affect the heart pressure at all. The unchanged venous heart-pressures during the three periods referred to suggest that the blood supply to the tissues during diving is kept on the same level as in the non-diving condition. Measurements of the pressures in the arterial heart and the arteries may provide further evidence on how—and where—the regulation takes place.

A full account of this work will appear elsewhere.

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April 9.

Late-glacial Finds of *Lepidurus arcticus* (Pallas) in the British Isles

IN 1894 Bennie[1] recorded abundant remains of this notostracan in association with arctic plant fossils at several localities near Edinburgh in deposits almost certainly of late-glacial age. He[2] found further specimens at Kirkmichael and near Ballaugh in the Isle of Man. There are also old records from Denmark and Sweden[3]. Many late-glacial deposits have been investigated in north-west Europe in recent years, but *Lepidurus arcticus* has not been found.

I am re-examining the Isle of Man deposits, and have made a special search for this fossil. Washings of full-glacial[4] age from Kirkmichael and of late-glacial (*sensu stricto*) age from near Ballaugh both yielded mandibles which were clearly those of a notostracan. As mandibles were common in the Ballaugh material at a stratigraphical level where Zone II was giving way to Zone III, further samples from that level were washed. Carapace fragments with sulcal spines, apodous segments and telsons with short supra-anal plates (Fig. 1) were found. This type of telson is characteristic of *Lepidurus arcticus* (Pallas)[5]. The fossil material is quite diaphanous, and easily escapes observation unless specially looked for. I sieved the samples with a U.S. 40 sieve. I then placed an aqueous suspension of the material retained on the sieve on a white plate, and inspected it in strong light with the aid of a 7× binocular dissecting microscope.

The same stratigraphical level is exposed in a late-glacial deposit at Neasham, Co. Durham[6], and samples from this deposit yielded notostracan mandibles, carapace fragments and apodous segments, but I did not find any telsons. Samples from Zone III at Mapastown, Co. Louth[7], yielded the same fossils. Re-examination of samples from other late-glacial localities would undoubtedly add many further identifications.

It is thus clear that *Lepidurus arcticus* was widely distributed in the British Isles in late-glacial time.

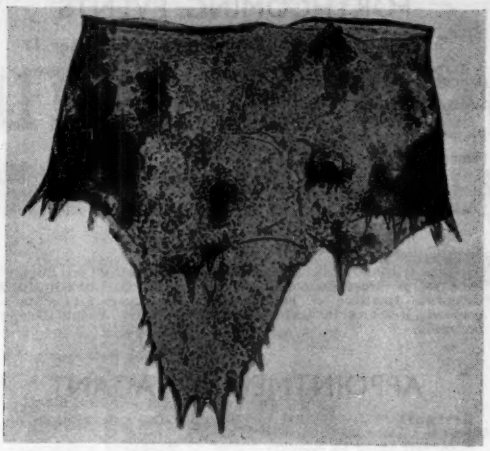

Fig. 1. Fossil telson with supra-anal plate of *Lepidurus arcticus* (Pallas): locality, near Ballaugh, Isle of Man; age, late-glacial, Zone II/III. × 50

To-day[8] there is a relict population in deep lakes in the Scandinavian mountains, where it descends to 58° N.; it descends to 57° N. in the Priblyoff Islands. But the main distribution is circum-polar, between 65° and 80° N., where it occurs in ponds and around shallow lake-margins, and is often very abundant. The late-glacial habitats must have been very similar.

G. F. MITCHELL

Department of Geology,
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May 15.

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Chirality

THE recent work on particle parity stresses the need for a suitable term for the property characterizing those three-dimensional forms which are distinct from their mirror images. 'Spirality', even if understood in the sense of a helix, is misleading, since many forms possess a non-superimposable mirror image but no helical axis; for example, scalene spherical triangles, isosceles tetrahedra with scalene faces, and certain types of optically active molecule. Kelvin[1], Larmor[2] and Eddington[3] have used the term 'chirality', from χείρ, hand. Chiral structures or fields are those which exist in two distinct mirror image forms, while non-chiral forms are single, possessing either a centre or a plane of symmetry.

L. L. WHYTE

93 Redington Road,
London, N.W.3.

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[2] "Aether and Matter", 142 (1900).
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FORTHCOMING EVENTS

Sunday, September 8—Wednesday, September 11

British Association for the Advancement of Science (at Dublin).—Continuation of Annual Meeting.

Sunday, September 8

At 11.15 a.m.—(in St. Patrick's Cathedral)—Religious Service. Preacher: The Lord Archbishop of Dublin.

Monday, September 9

At 10 a.m.—Dr. G. H. Mitchell, F.R.S.: "Ordovician Volcanoes" (Presidential Address, Section C).

At 8 p.m.—Sir Sydney Caine: "Education for Development".

Friday, September 13

Royal Aeronautical Society (at the Institution of Civil Engineers, Great George Street, London, S.W.1), at 6 p.m.—Dr. Clark B. Millikan (California Institute of Technology): "Advanced Education and Academic Research in Aeronautics" (45th Wilbur Wright Memorial Lecture).

APPOINTMENTS VACANT

Applications are invited for the following appointments on or before the dates mentioned:

Scientist (with an honours degree, or equivalent qualification, in chemistry or physics, preferably with experience in the coalmining industry and with wide analytical experience and administrative ability) as assistant to the Area Chief Scientist in the North Staffordshire Area Laboratory, Stoke on Trent—Area Staff Manager, West Midlands Divisional Coal Board, 72 Leek Road, Stoke-on-Trent (September 13).

Technical Assistant (preferably with some knowledge of photographic techniques) in the Department of Botany, University of Natal, Pietermaritzburg—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (South Africa, September 15).

Chief Technician in the Department of Zoology, University College of Ghana—The Assistant Registrar (London), University College of Ghana, 29 Tavistock Square, London, W.C.1 (September 16).

Scientific Officer (with an honours degree in agriculture or agricultural botany, or equivalent qualification, with a sound knowledge of the principles of genetics and practical plant breeding, preferably with experience in design and analysis of field trials) in the Plant Breeding Division, Ministry of Agriculture, Loughall, Co. Armagh—The Secretary, Civil Service Commission, Stormont, Belfast (September 18).

Principal (with a degree in engineering, or professional equivalent, with technical college teaching and administrative experience, also some industrial experience) of a new Technical Institute, Ibadan, to administer, organize and assist in the establishment of a new Institute, and to correlate administration of technical institute with trade training centre—The Director of Recruitment, Colonial Office, Great Smith Street, London, S.W.1, quoting BCD.114/410/016 (September 27).

Professor (with a university degree in science or agriculture, with special qualifications and experience in teaching and research) of Plant Science (Agricultural Botany) at the Canterbury Agricultural College, New Zealand—The Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 (September 30).

Lecturer, Grade I or II, in Low Temperature Physics, to give instruction in physics and to lead the low temperature research group under the direction of the Professors of Physics—The Registrar, The University, Bristol 8 (October 1).

Lecturer or Assistant Lecturer in the Rheumatism Research Centre, Clinical Section—The Registrar, The University, Manchester 13 (October 1).

Lecturer and Assistant Lecturer in Psychology—The Registrar, The University, Hull (October 7).

Instructor or Assistant Professor (with Ph.D. degree, or equivalent qualification, and some university teaching experience) in Entomology—Prof. I. McT. Cowan, Department of Zoology, University of British Columbia, Vancouver 8, B.C., Canada.

Mathematician (with a university degree and preferably some experience of statistics), for work initially concerned with the statistical analysis of data relating to the structural strength and performance of ships, and later with more general problems in the fields of strength, vibration and hydrodynamics—The British Shipbuilding Research Association, 5 Chesterfield Gardens, Curzon Street, London, W.1.

Research Assistant (with a first- or second-class honours degree in biochemistry, or in chemistry and physiology) in the Pharmacology Department—The Secretary, St. Mary's Hospital Medical School, Paddington, London, W.2.

Senior Lecturer in Animal Husbandry at the University College of Rhodesia and Nyasaland—The Secretary, Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C.1.

Systematic Botanist (with D.Sc. or Ph.D. degree in botany, preferably systematic botany, and a knowledge and experience of subtropical plants) with the Commonwealth Relations Office for a post in Pakistan. Duties include research work in establishing a national herbarium for the preparation of flora of Pakistan, outlining work on ecological, physiological medicinal plants aspects, also genetics, to be responsible for related work in the Pakistan Forest Research Institute and to act as consultant in organization of research and teaching in botany, possibly also to assist in training of personnel and teaching—The Ministry of Labour and National Service (E.9), Almack House, King Street, London, S.W.1, quoting A.12/PAK/TCS/822.

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Indian Forest Bulletin. (New Series.) No. 185: Silviculture—Cover and Nurse Crops in Sal and Teak Plantations at Dehra Dun. By V. S. Krishnaswamy. Pp. 17. Rs. 1/12/-; 2*s*. 6*d*. No. 198: Wood Seasoning—The Seasoning Behaviour of Indian Timbers. Part 3: Kiln Drying Schedules and Seasoning Peculiarities of some Indian Woods. By M. A. Rehman. Pp. 8. As. 7; 8*d*. (Delhi: Manager of Publications, 1956.) [87

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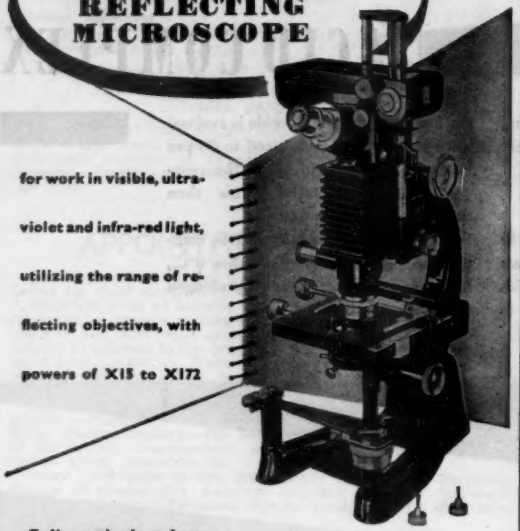

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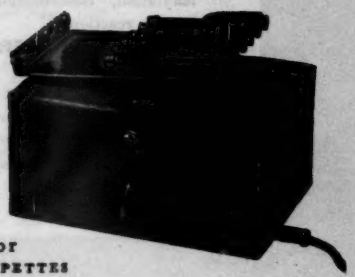

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